Nepotism vs. Intergenerational Transmission of Human Capital in Academia (1088–1800)
Online appendix

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A Data appendix

This appendix lists the 307 most important secondary sources used to construct our dataset of 1,538 and 1,748 in universities and academies between 1088 and 1880. First, we complement the description in the main text on the coverage and accuracy of the data by providing additional summary statistics. Next, we describe in detail the secondary sources used for the largest universities and academies in our dataset and list all the data sources used for each institution in our dataset. Finally, we presents two examples: one to illustrate multiple-generation lineages of scholars (the Chicoyneau and Mögling dynasties), another to illustrate our data collection process (Honoré Bicais and his son Michel).

A.1 Additional descriptives on data coverage

As explained in Section 2 of the main text, we distinguish three levels of completeness in the sources used to construct our dataset of father-son pairs in pre-industrial academia (1088–1880):

- A *partial* coverage describes a situation in which the sample of scholars in an institution was informed by sources from other universities and general thematic biographies only. Under partial coverage, there is risk of sampling bias: On the one hand, if a scholar had a father who was briefly or no great account a professor, this is more likely to fall by the wayside than an underachieving son of a famous professor. On the other hand, if a scholar had a son who was briefly or no great account a professor, this is more likely to fall by the wayside than an underachieving father of a famous professor.
- A *broad* coverage is for father-son pairs in institutions where a members' catalogue listing all scholars in that institution does not exist. Instead, we use a combination of sources covering that particular institution, for example, a book on the history of that particular university or academy. Sources

with broad coverage cover a large sample of scholars in an institution. Under broad coverage, hence, sampling bias is less likely, although we cannot fully rule it out.

- A *complete* coverage is for father-son pairs in institutions that are covered by an existing catalogue, compendium, website, or book whose aim is to list all scholars in that institution. For example, a source with complete coverage is a catalogue of all professors in a particular university or academy. Under complete coverage it is possible to distinguish whether a scholar's father was a professor or not with certainty.

Table A.1 shows the number of institutions and the number of father-son pairs by each coverage category. Two thirds of our father-son pairs are from sources with complete coverage, 95.5 percent from sources with complete and broad coverage, and only 5.4 percent from sources with partial coverage. At the institution level, about half of the universities and academies included in our dataset have secondary sources with complete coverage, and 80 percent have secondary sources with complete and broad coverage. Importantly, the quality of the coverage is not related to the prestige of the university. We have an excellent coverage of the University of Macerata – a small university in Italy, while there is no comprehensive list of professors for the University of Paris.

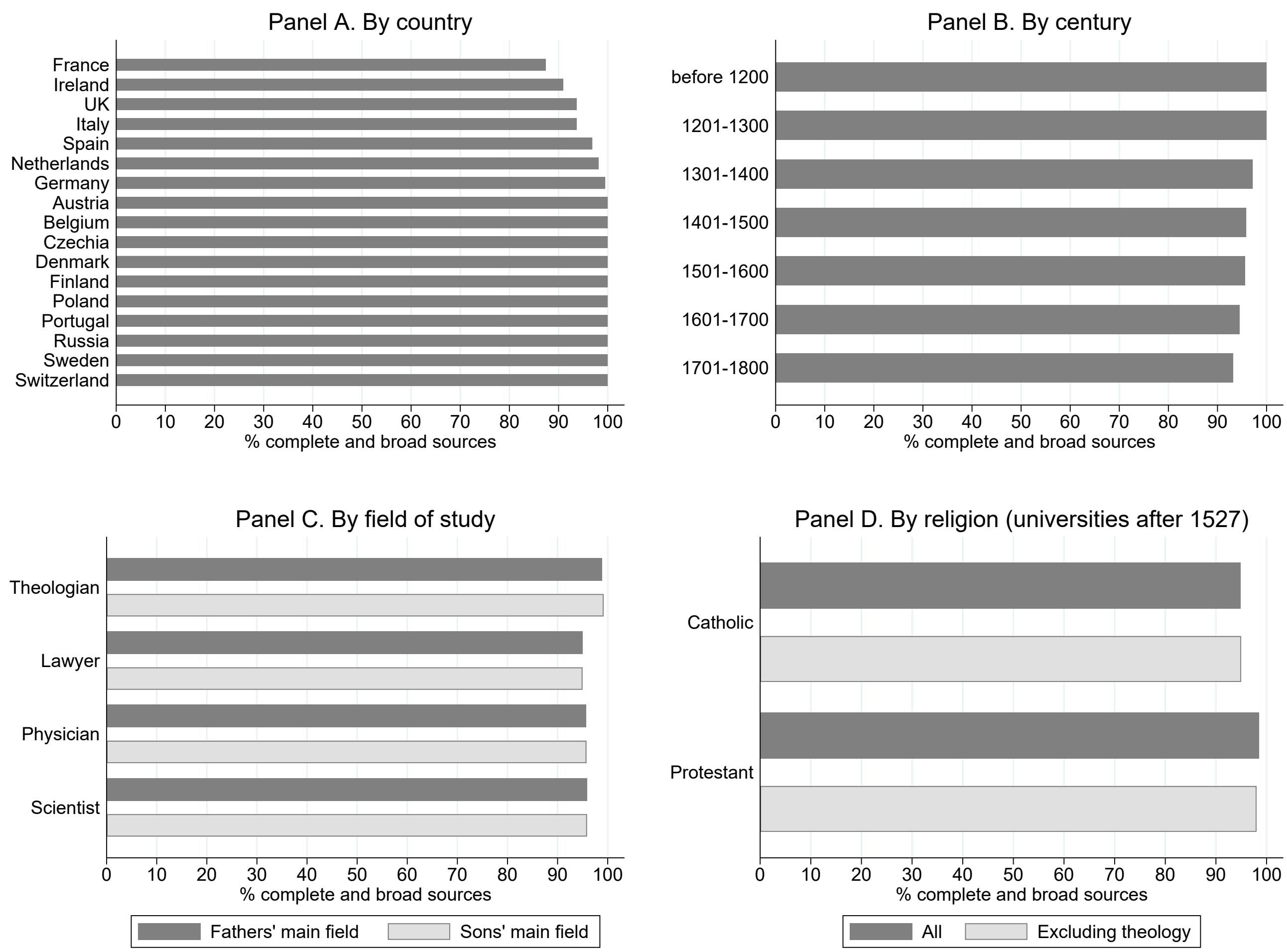
Table A.1: Breadth of coverage

Coverage	Number of institutions	Number of sons
Complete	80	1,134
Broad	56	520
Partial	30	94
Total	166	1,748

Next, we show that the share of father-son pairs coded from better sources is not heterogeneous across time, space, field of study, and religion. Specifically, Panel A of Figure A.1 shows the percentage of father-son pairs under complete and broad coverage by the country where the university or academy is located. Countries are based on modern borders. There is very little variation in this percentage, which ranges from ca. 90 to 100%. Note also that the countries where the percentage of father-son pairs from complete and broad sources is below 100 percent are both from north-west (e.g., UK) and southern Europe (e.g., Italy), and include both catholic and protestant countries.

In the main text, Table 1 showed that the coverage of the sources used to identify father-son pairs was stable across the four historical periods in our analysis: the period before 1543, the beginning of the Scientific Revolution (1543-1632), the second part of the Scientific Revolution (1633-1687), and the Enlightenment (1688-1800). Panel B of Figure A.1 complements this evidence by showing that the share

Figure A.1: Percent of father-son pairs recovered from sources with complete and broad coverage, by country, century, field of study, and religion.



of father-son pairs identified from better sources, by century. Specifically, it sorts father-son pairs by centuries based on the fathers' reference date—which includes a combination of their birth year, nomination year, or approximate activity year. The figure shows that the percentage of father-son pairs under complete and broad coverage is always above 90 percent, independenty of the century.

Similarly, Panel D of Figure A.1 shows that fathers and sons in the main fields of study that we consider in our analysis—theology, law, medicine (physicians), and science—are recovered from data sources of similar quality. As before, the percentage of fathers and sons under sources with complete and broad coverage varies little across their respective fields of study.

Finally, Panel E of Figure A.1 shows the breadth of the coverage by religion. We consider the religion of Universities after 1527—when the first Protestant University was established in Marburg. In both catholic and protestant universities, around 90 percent of father-son pairs are based on sources with complete and broad coverage. We obtain a very similar result when we exclude theology scholars, who were typically priests or pastors and, hence, could only have (legitimate) descendants in protestant universities.

Altogether, this evidence shows that our main results, our results over time, and or heterogeneity analysis are based on sources with very good coverage, where the possibility of selective reporting of father-son links is unlikely. In other words, it is unlikely that our estimates are driven by sampling bias in the father-son links, or by composition effects where the groups compared are based on data sources with different coverage and accuracy. Nevertheless, to fully rule out the possibility of sampling bias, in the main text we examine the robustness of our results to using data with complete coverage alone.

A.2 Data sources

Table A.2 summarizes the ten institutions with more father-son pairs in our dataset. Table A.3 lists the secondary sources used for each of the 115 universities and 51 scientific academies included in our database. Specifically, the table provides the name of the university or academy, its foundation date (and, when applicable, closure date), the number of father-son pairs in that institution, the references for the secondary sources used, and the coverage of these sources (3 =Complete, 2 =Broad, 1 =Partial).

TABLE A.2: Institutions with the largest number of father-son pairs.

Institution (dates)	N	Main Sources	Biographical dictionary[†]
U. Bologna (1088-)	171	Mazzetti (1847)	Treccani
Royal Society (1660-)	78	www.royalsociety.org/	DNB
Accademia dei Ricovrati (1599-)	61	Maggiolo (1983)	Treccani
U. Padova (1222-)	59	Facciolati (1757), Del Negro (2015)	Treccani
U. Avignon (1303-1793)	58	Laval (1889), Fournier (1892) Teule (1887), Duhamel (1895)	Barjavel (1841)
U. Copenhagen (1475-)	47	Slottved (1978)	www.geni.com
U. Tübingen (1476-)	47	Conrad (1960)	ADB
U. Basel (1460-)	45	Herzog (1780)	Attinger (1928)
Leopoldina (1652-)	44	www.leopoldina.org/	ADB
U. Montpellier (1289-1793)	37	Dulieu (1975, 1979, 1983)	Clerc (2006)

Notes: ADB: Allgemeine Deutsche Biographie; DNB: Dictionary of National Biography; Treccani: Enciclopedia italiana; N: number of father-son pairs; [†]Main biographic dictionary used.

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (1/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage
University of Bologna	Bologna	ITA	1088		171	Mazzetti (1847)	3
Royal Society of London (· · ·)	London	GBR	1660		78	https://royalsociety.org/	3
Accademia dei Ricovrati	Padova	ITA	1599		61	Maggiolo (1983)	3
University of Padua	Padova	ITA	1222		59	Pesenti (1984), Casellato and Rea (2002), Facciolati (1757),Del Negro (2015)	3
University of Avignon	Avignon	FRA	1303	1793	58	Laval (1889), Fournier (1892), Teule (1887), Duhamel (1895), Barjavel (1841)	2
University of Copenhagen	København	DNK	1475		47	Slottved (1978)	3
University of Tubingen	Tübingen	DEU	1476		47	Conrad (1960)	3
University of Basel	Basel	CHE	1460		45	Herzog (1780), Junius Institute (2013) Rosen (1972)	2
Academy of Sciences Leopoldina	Halle	DEU	1652		44	http://www.leopoldina.org/	3
University of Montpellier	Montpellier	FRA	1289	1793	37	Astruc (1767), Dulieu (1975, 1979, 1983)	2
University of Leipzig	Leipzig	DEU	1409		31	Hehl (2017)	3
University of Cambridge	Cambridge	GBR	1209		31	Walker (1927), Venn (1922)	2
University of Jena	Jena	DEU	1558		30	Günther (1858)	3
Univ. of Pavia	Pavia	ITA	1361		28	Raggi (1879)	3
University of Marburg	Marburg	DEU	1527		25	Gundlach and Auerbach (1927)	3
University of Greifswald	Greifswald	DEU	1456		24	https://www.uni-greifswald.de	2
University of Giessen	Gießen	DEU	1607		24	Haupt and Lehnert (1907)	3
University of Helmstedt	Helmstedt	DEU	1575	1809	22	Gleixner (2019)	3
Royal Prussian Academy of Sciences	Berlin	DEU	1700		22	BBAW (2019)	3
French Academy of Sciences	Paris	FRA	1666	1793	22	Maury (1864), Rozier (1776)	3
University of Strasbourg	Strasbourg	FRA	1538		22	Berger-Levrault (1890)	3
University of Paris	Paris	FRA	1200	1793	19	Antonetti (2013), Courtenay (1999), Hazon and Bertrand (1778)	2
University of Rostock	Rostock	DEU	1419		19	Krüger (2019)	3
University of Königsberg	Kaliningrad	RUS	1544		19	Naragon (2006), Schwinges and Hesse (2019)	2
University of Leiden	Leiden	NLD	1575		17	Leiden (2019)	3

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (2/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage
University of Lund	Lund	SWE	1666		17	Tersmeden (2015),Delen and Weibull (1868)	3
University of Wittenberg	Wittenberg	DEU	1502	1813	17	Kohnle and Kusche (2016)	2
University of Perugia	Perugia	ITA	1308		16	Frova and Zucchini (2017),Zucchini (2008)	2
University of Edinburgh	Edinburgh	GBR	1582		15	Junius Institute (2013), Grant (1884)	3
University of Geneva	Genève	CHE	1559		14	Junius Institute (2013), Borgeaud (1900)	3
Académie royale d'architecture	Paris	FRA	1671	1793	14	`www.cths.fr`	1
Académie Royale (· · ·) de Lyon	Lyon	FRA	1700	1790	13	Dominique (2017)	3
Collège Royal	Paris	FRA	1530		13	Collège de France, (2018)	3
University of Halle	Halle (Saale)	DEU	1694	1817	13	Schopferer (2016)	3
University of Pisa	Pisa	ITA	1343		13	Fabroni (1791)	3
University of Salamanca	Salamanca	ESP	1218		13	Addy (1966),Arteaga (1917), Vidal y Díaz et al. (1869)	3
Academy of(· · ·) Mainz	Erfurt	DEU	1754		12	Kiefer (2004)	3
Royal Swedish Academy of Sc.	Stockholm	SWE	1739		12	`http://www.kva.se`, Dahlgren (1915)	3
University of Valence	Valence	FRA	1452	1793	12	Brun-Durand (1901), Nadal (1861)	2
University of Louvain	Leuven	BEL	1425	1797	12	Ram (1861), Nève (1856),Brants (1906), Lamberts and Roegiers (1990)	2
Heidelberg University	Heidelberg	DEU	1386		12	Drüll (1991), Drüll (2002)	3
University of Aix	Aix-en-Provence	FRA	1409	1793	11	Belin(1896, 1905), Fleury and Dumas (1929), de la Croix and Fabre (2019)	2
Accademia Fiorentina	Firenze	ITA	1540	1783	11	Boutier (2017)	1
University of Oxford	Oxford	GBR	1200		11	Emden (1959), Foster (1891)	2
University of Lausanne	Lausanne	CHE	1537		11	Junius Institute (2013),Kiener and Robert (2005)	3
University of Franeker	Franeker	NLD	1585	1811	10	Feenstra, Ahsmann, and Veen (2003)	2
University of Cahors	Cahors	FRA	1332	1751	10	Ferté (1975)	2
Royal Society of Edinburgh	Edinburgh	GBR	1783		10	RSE (2006)	3
Académie des inscriptions (· · ·)	Paris	FRA	1663		10	Boutier (2018)	2
Collegium Carolinum	Zurich	CHE	1525		10	Junius Institute (2013), Attinger, Godet, and Türler (1928)	2

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (3/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage
University of Poitiers	Poitiers	FRA	1431	1793	10	Boissonade (1932)	2
University of Toulouse	Toulouse	FRA	1229	1793	10	Deloume (1890), Barbot (1905), Lamothe-Langon (1823)	2
University of Kiel	Kiel	DEU	1652		10	Volbehr and Weyl (1956)	3
University of Bordeaux	Bordeaux	FRA	1441	1793	10	Gaullieur (1874),Pery (1888)	2
University of Salerno	Salerno	ITA	1231		9	Sinno (1921), De Renzi (1857)	2
Uppsala University	Uppsala	SWE	1477		9	Von Bahr (1945), Astro.uu.se (2011), Jensen (2018)	2
Zamojski University	Zamosc	POL	1594	1784	9	Kedzoria (2021)	2
University of Göttingen	Göttingen	DEU	1734		8	Ebel (1962)	3
University of Angers	Angers	FRA	1250	1793	8	Rangeard and Lemarchand (1868), de Lens (1880), Denéchère and Matz (2012), Port (1876)	2
Jagiellonian University	Krakow	POL	1364		8	Pietrzyk and Marcinek (2000), `http://www.archiwum.uj.edu.pl/`	3
Societas Privatas Taurinensis	Torino	ITA	1757	1792	8	Accademia delle Scienze di Torino (1973)	3
Utrecht University	Utrecht	NLD	1636		7	Dorsman (2011)	3
Jardin Royal des Plantes	Paris	FRA	1635	1793	7	Jaussaud and Brygoo (2004)	3
Academy of St Petersburg	Saint-Petersburg	RUS	1724	1917	7	Shemivot (1873)	3
University of Siena	Siena	ITA	1246		7	Frova, Catoni, and Renzi (2001)	2
French Academy	Paris	FRA	1635		7	`http://www.academie-francaise.fr/`	3
University of Groningen	Groningen	NLD	1612		7	`https://hoogleraren.ub.rug.nl/`	3
University of Torino	Torino	ITA	1404		7	Vallauri (1875)	2
Åbo Akademi University	Turku	FIN	1640		7	Klinge et al. (1988)	2
University of Pont-à-Mousson	Pont-à-Mousson	FRA	1572	1768	7	Martin (1891)	3
Dutch Academy of Sciences	Haarlem	NLD	1752	1804	7	`https://khmw.nl/historische-leden/`	3
University of Florence	Firenze	ITA	1321	1515	7	Prezziner (1810), Gherardi (1881)	2
Royal Spanish Academy	Madrid	ESP	1713		6	`https://www.rae.es/la-institucion/`	3
University of Perpignan	Perpignan	FRA	1350	1793	6	Carmignani (2017), Capeille (1914),Izarn (1991)	2
Bavarian Academy of (· · ·)	München	DEU	1759		6	Bayerische Akademie der Wissenschaften (2022)	3
University of Nantes	Nantes	FRA	1460	1793	6	Chenon (1890), Grünblatt (1961)	2

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (4/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage
"Mersenne" Academy	Paris	FRA	1635	1648	6	de Coste (1649)	3
University of Dublin	Dublin	IRL	1592		6	Kirkpatrick (1912), Burtchaell and Sadleir (1935)	2
University of Caen	Caen	FRA	1432	1793	6	de Pontville (1997a)	1
University of Ingolstadt	Ingolstadt	DEU	1459	1800	6	Wolff (1973)	3
Académie des Sciences et B.-L.	Bordeaux	FRA	1712	1793	6	Courteault (1912)	3
Académie des Sciences et B.-L.	Toulouse	FRA	1729	1793	6	Taillefer (1985)	3
University of Dole	Dole	FRA	1422	1793	6	Beaune and d'Arbaumont (1870)	2
Old University of Aberdeen	Aberdeen	GBR	1495		6	Anderson (1893)	3
University of Glasgow	Glasgow	GBR	1451		5	Coutts (1909), University of Glasgow (2020)	2
Royal College of Physicians	London	GBR	1518		5	Munk (1878)	1
University of Modena	Modena	ITA	1175		5	Mor and Di Pietro (1975)	2
University of Valladolid	Valladolid	ESP	1280		5	Alcocer Martinéz (1918)	3
Accademia della Crusca	Firenze	ITA	1583		5	Parodi (1983)	3
University of Ferrara	Ferrara	ITA	1391		5		1
New University of Aberdeen	Aberdeen	GBR	1593		5	Anderson (1898)	3
Majorcan cartographic school	Palma	ESP	1330	1500	5	Pastor and Camarero (1960)	2
Accademia delle scienze	Bologna	ITA	1714		5	Ercolani (1881)	3
University of Rinteln	Rinteln	DEU	1620	1809	4	Hänsel (1971)	3
University of Mainz	Mainz	DEU	1476	1792	4	Benzing (1986)	3
Académie des Arts et B.-L.	Caen	FRA	1705	1793	4	de Pontville (1997b)	3
University of St Andrews	Saint-Andrews	GBR	1411		4	Smart (2004)	2
Académie (.) de la Rochelle	La Rochelle	FRA	1732	1744	4	Marion (2019)	2
Universty of Naples	Napoli	ITA	1224		4	Origlia Paolino (1754)	2
University of Montauban	Montauban	FRA	1598	1659	4	Bourchenin (1882)	3
University of Sedan	Sedan	FRA	1599	1681	4	Bourchenin (1882)	3
Académie d'agriculture	Paris	FRA	1761	1793	4	Marion (2019)	1
Akademisches Gymnasium Danzig	Gdansk	POL	1558		4	Hirsch (1837)	3
Royal Dublin Society	Dublin	IRL	1757		4	Berry (1915)	3
University of Altdorf	Altdorf bei Nürnberg	DEU	1578	1809	3	Flessa (1969)	2

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (5/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage
Société Royale des Sciences	Montpellier	FRA	1706	1793	3	Dulieu (1983)	3
University of Harderwijk	Harderwijk	NLD	1647	1811	3	van Epen (1904)	3
Braunschweig University (···)	Braunschweig	DEU	1745		3	Albrecht (1986)	3
University of Orléans	Orléans	FRA	1235	1793	3	Bimbenet (1853), Duijnstee (2010)	2
University of Rome	Roma	ITA	1303		3	Renazzi (1803)	2
Gottingen Academy of Sciences	Göttingen	DEU	1752		3	Krahnke (2001)	3
University of Catania	Catania	ITA	1444		3	Sabbadini (1898)	2
University of Douai	Douai	FRA	1559	1793	3	Collinet (1900)	2
Académie des Sciences, ...	Clermont-Ferrand	FRA	1759	1793	3	Mège (1999)	1
University of Saumur	Saumur	FRA	1596	1685	2	Bourchenin (1882)	3
Académie des belles-lettres, (···)	Marseille	FRA	1726	1793	2	`http://www.academie-sla-marseille.fr/`	2
University of Würzburg	Würzburg	DEU	1402		2	Walter (2010)	2
Viadrina European University	Frankfurt O.	DEU	1506	1811	2	Junius Institute (2013)	1
Universite of Die	Die	FRA	1601	1684	2	Bourchenin (1882)	3
University of Macerata	Macerata	ITA	1540		2	Serangeli (2010)	3
Academy of Gorlitz	Gorlitz	DEU	1773		2	`https://www.olgdw.de/gesellschaft/`	3
Agriculture Society of Lyon	Lyon	FRA	1761		2	Marion (2019)	1
University of Erlangen	Erlangen	DEU	1742		2	Wachter (2009)	3
Danzig Research Society	Gdansk	POL	1743	1936	2	Schumann (1893)	3
University of Vienna	Wien	AUT	1465		2	Lackner (1976), Schwinges and Hesse (2019), von Aschbach (1865)	2
Literary and philosophical society	Manchester	GBR	1781		2	Anonymous (1896)	3
University of Parma	Parma	ITA	1412		2	Rizzi (1953)	2
Academy of the Unknown	Venezia	ITA	1626	1661	1	British Library Board (2017)	1
Athenaeum Illustre of Amsterdam	Amsterdam	NLD	1632	1877	1	`http://www.albumacademicum.uva.nl/`	3
Academy of the Burning Ones	Padova	ITA	1540	1545	1	British Library Board (2017)	1
Freiberg University (···)	Freiberg	DEU	1765		1	Appelt and Wulkow (2022)	3
Nijmegen University	Nijmegen	NLD	1655	1679	1		1
Veneziana (Seconda Accademia)	Venezia	ITA	1594	1608	1	British Library Board (2017)	2

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (6/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage
University of Nîmes	Nîmes	FRA	1539	1663	1	Bourchenin (1882)	3
University of Moscow	Moskow	RUS	1755		1	Andreev and Tsygankov (2010)	3
Academy of the Invaghiti	Mantova	ITA	1562	1738	1	British Library Board (2017)	2
University of Rennes	Rennes	FRA	1735	1793	1	Chenon (1890)	3
University of Freiburg	Freiburg	DEU	1457		1	Bauer (1957)	2
University of Prague	Prague	CZE	1348		1	Svatoš and Čornejová (1995), Čornejová and Fechtnerová (1986)	2
University of Erfurt	Erfurt	DEU	1379		1	Schwinges and Hesse (2019)	1
Royal Botanic Garden	Kew	GBR	1759		1		1
Academie de Beziers	Béziers	FRA	1723	1793	1	Marion (2019)	1
University of Cervera	Cervera	ESP	1714	1821	1	Rubio y Borras (1914)	3
Academy of the Umorists	Roma	ITA	1603	1670	1	British Library Board (2017)	2
Academy of the Filateri	Ferrara	ITA	1554	1563	1	British Library Board (2017)	2
University of Bourges	Bourges	FRA	1464	1793	1		2
University of Orange	Orange	FRA	1365		1		1
Society of Observers of Man	Paris	FRA	1799	1804	1		1
University of Oviedo	Oviedo	ESP	1574		1	Canella Secades (1873)	1
Royal Danish Science Society	Copenhagen	DNK	1742		1	Lomholt (1950)	3
French Academy of Medecine	Paris	FRA	1731	1793	1		1
University of Duisburg	Duisbrug	DEU	1654		1		1
University of Cagliari	Cagliari	ITA	1606		1	Tola (1837), Pillosu (2017)	2
Accademia Roveretana	Rovereto	ITA	1752		2		1
University of Alcala	Alcala de H.	ESP	1499		1	Torrecilla, Arboniés, and Torres (2013)	2
University of St Petersburg	St. Petersburg	RUS	1724		1	Shemivot (1873)	2
Philosophical Society	Dublin	IRL	1683	1778	1	Wilde and Lloyd (1844)	1
Accademia Botanica	Firenze	ITA	1739	1783	1		1
University of	Dijon	FRA	1722	1792	1		1
University of Fermo	Fermo	ITA	1585		1	Brizzi (2001), Curi (1880)	2
University of Coimbra	Coimbra	PRT	1308		1		2

Table A.3: Sources used, number of father-son pairs, and coverage, by institution (7/7)

Institution	City	Country	Dates		Nb.	Sources	Coverage
University of Pau	Pau	FRA	1722	1793	1	Maisonnier (1972)	1
University of Besancon	Besancon	FRA	1691	1793	1	Lavillat (1977)	3
University of Nancy	Nancy	FRA	1768	1793	1		1
Académie des sciences, ...	Angers	FRA	1685	1793	1	Marion (2019)	2
University of Mondovi	Mondovi	ITA	1560		1	Vallauri (1875)	1

Notes: Missing sources correspond to families which were mentioned in sources about other institutions.
Coverage: 3=Complete, 2=Broad, 1=Partial.

A.3 Examples

Multi-generation lineages of scholars. Our database contains 171 families with three or more generations of scholars at the same university or scientific academy. For the sake of illustration, Figure A.2 shows one of these dynasties of scholars: the Chicoyneau. The Chicoyneaus had four generations of scholars, all employed at the University of Montpellier. For almost a century (from 1659 to 1758), there was at least one Chicoyneau at the University of Montpellier. This lineage was reconstructed using Dulieu (1983). Note that some Chicoyneaus developed a prolific career. For example, François Chicoyneau (1672-1752) was a professor at Montpellier and was also appointed at the Académie des Sciences. Other members of the dynasty were appointed professor at very early ages. The last member of the dynasty, Jean-François Chicoyneau (born in 1737), was made a professor in 1752—that is, at the tender at age of 15. In principle, dynasties like the Chicoyneaus may emerge because human capital was strongly transmitted across generations, because of nepotism, or because of a combination of both.

Similarly, Figure A.3 displays another multi-generation lineage of scholars: the Mögling family at the University of Tübingen (Conrad 1960). This lineage spans six generations, from the sixteenth to the eighteenth century. The first three generations were professors in medicine. After Johan David Mögling (1650-1695), however, the family switch to law (in section 6.3 of the main text, we exploit such field switches). In the first and fifth generation, the lineage members held a professorship elsewhere: Daniel Mögling (1546-1603) at Heidelberg, Johan Friedrich Mögling (1690-1766) at Giessen.

In the main text, we exploit these multi-generation lineages to address measurement error in estimates for the transmission of human capital. Specifically, we use multi-generation lineages to compute correlations in observed publications across multiple generations. Elsewhere it has been shown that, under the assumption that measurement error is constant across generations, these multi-generation correlations reflect the transmission of (unobserved) underlying human-capital endowments. In other words, multi-generation lineages help us tackle the measurement error bias in parent-child publication elasticities.

Data collection example - Honoré and Michel Bicais. In Section 2 on the main text, we illustrate the data collection process by using the example of Honoré Bicais and his son Michel, both professors at the University of Aix. Figure A.4 shows the different sources mentioned in the main text: **(a)** Honoré Bicais' biography from Belin's *Histoire de l'Ancienne Universite de Provence* (1905) — used to identify Honoré (and Michel) as professors at the University of Aix; **(b)** The biographical dictionary of Aix's Department, *Les Bouches-du-Rhône, Encyclopédie Départementale* by (Mason 1931) — used to retrieve birth years and the quote that Michel Bicais succeeded his father in "in his chair and in his reputation;" and **(c)** Honoré and Michel Bicais' WorldCat entries — used to measure their scientific output in the form of library holdings by or about them in modern libraries.[1]

[1]The WorldCat entries in Figure A.4 were accessed on 30th of November, 2020. The number of library holdings may change slightly if modern libraries acquire/retire copies of the works by or about these authors.

Figure A.2: The Chicoyneau dynasty.

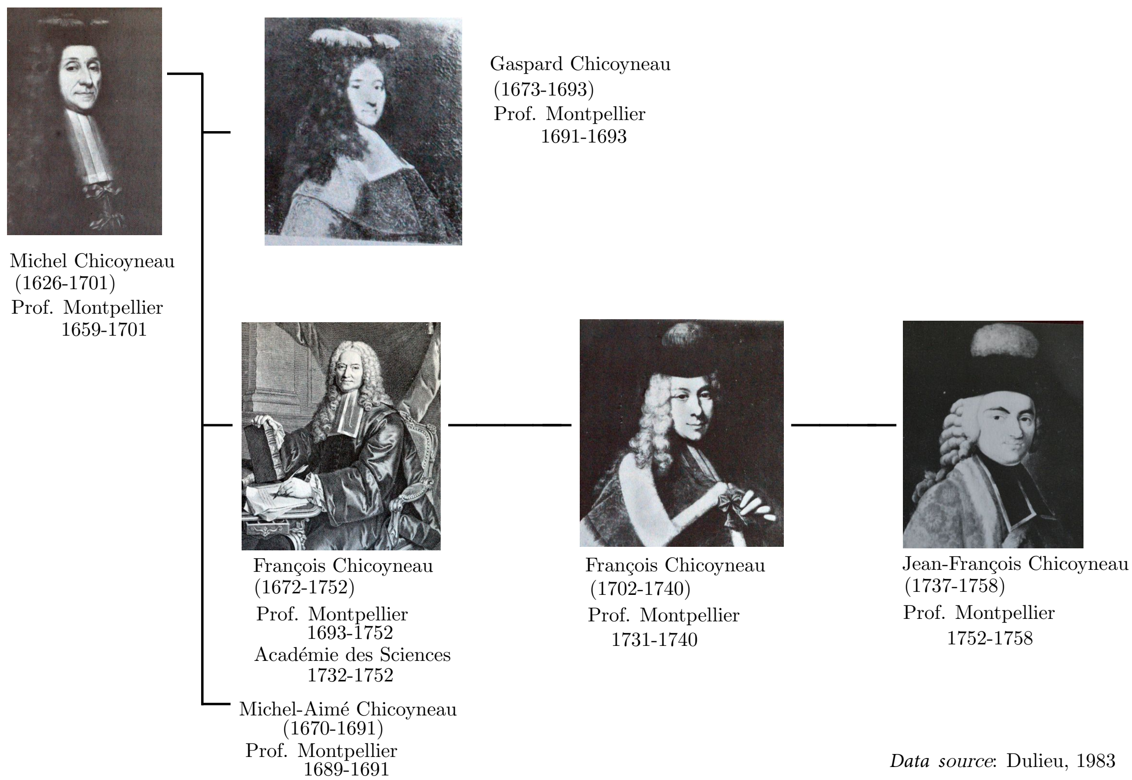


Figure A.3: The Mögling dynasty.

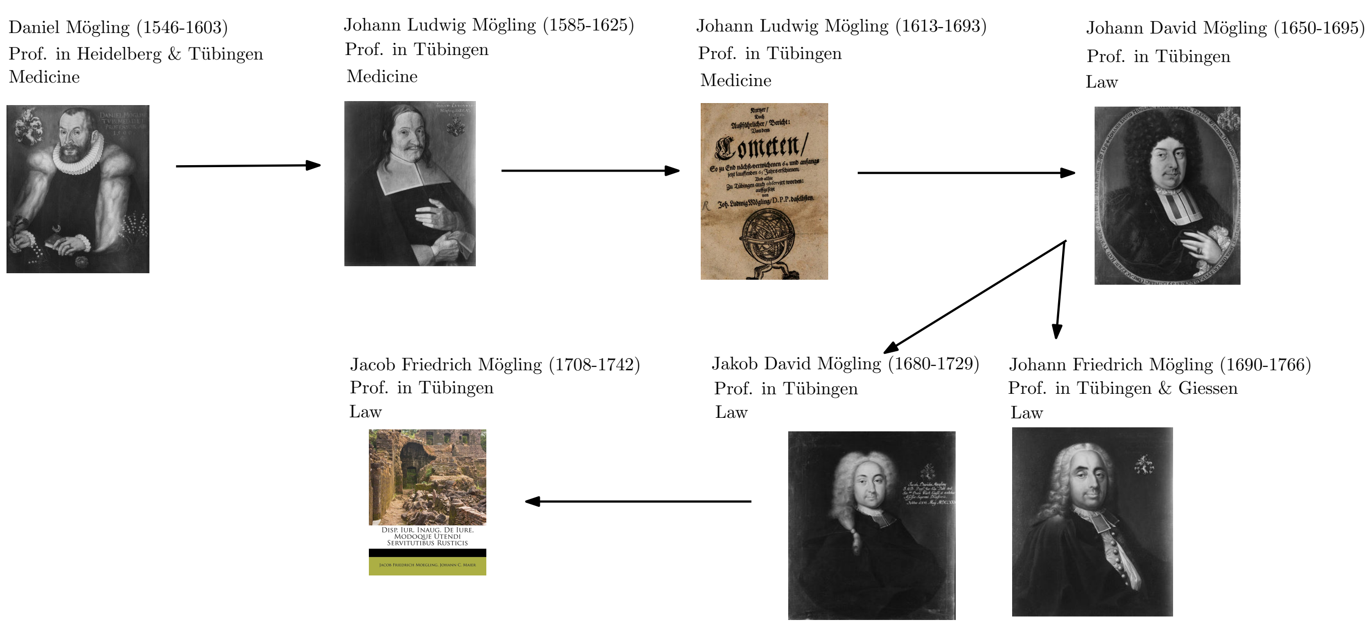
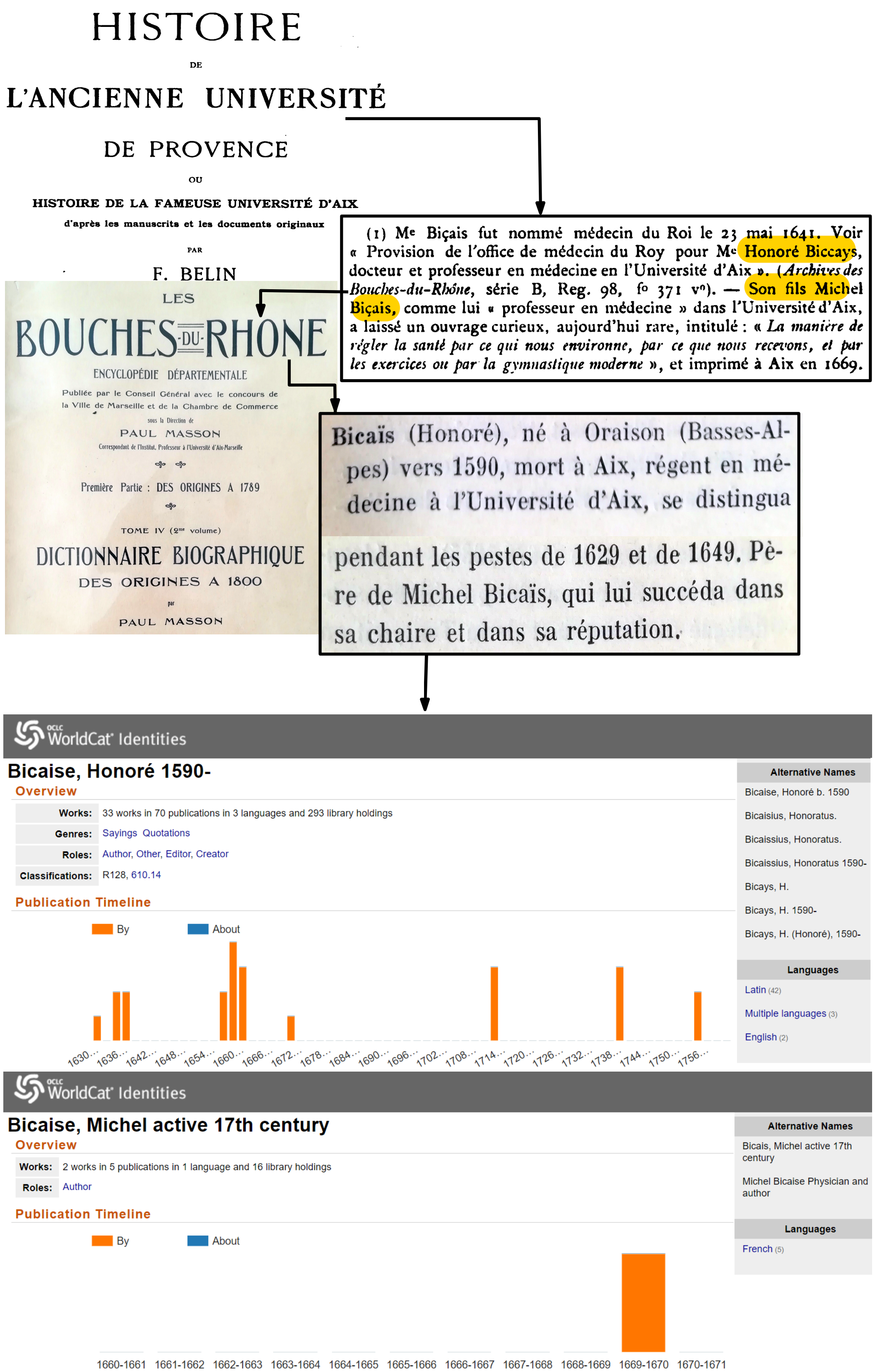


Figure A.4: Example of data collection - Honoré and Michel Bicais



B Intergenerational estimates in the literature

This appendix describes existing methods in the literature to estimate intergenerational elasticities, and highlights two potential biases: measurement error and selection of families in the data.

Parent-child elasticities. To study the extent to which inequalities are transmitted across generations, economists typically estimate coefficient b in:

$$y_{i,t+1} = b\, y_{i,t} + e_{i,t+1}\,, \tag{1}$$

where i indexes families, t parents, and t+1 children. The outcome y reflects social status (e.g., income, wealth, education, occupation) and is in logarithms. The coefficient b is the intergenerational elasticity of outcome y. It determines the speed at which outcomes revert to the mean. To see this, note that the half-life of y (the generations until the gap to the mean halves) is $t_{\frac{1}{2}} = -\ln(2)\ /\ \ln(|b|)$, which depends negatively on b.

Panel A of Table B.4 shows estimates of b in the literature.[2] Parent-child elasticities vary across time and space, but are generally below 0.5. This implies a half-life of $t_{\frac{1}{2}} = 1$. That is, half the gap to the mean is filled after one generation. In three generations, almost all advantages will revert to the mean.

Measurement error bias. Recent studies looking at multiple generations show that social status is more persistent than suggested by parent-child elasticities. One possible reason is that there is a highly-persistent inherited endowment that wealth, income, or occupation only reflect noisily. Children do not inherit their socio-economic outcomes directly from their parents. Instead, children inherit an unobserved human capital endowment h (e.g., knowledge, skills, genes, preferences) which then transforms into the observed outcome y imperfectly. This is modelled as a first-order Markov process of endowments transmission where endowments are observed with measurement error (Clark and Cummins 2015; Braun and Stuhler 2018):

$$h_{i,t+1} = \beta h_{i,t} + u_{i,t+1}\,, \tag{2}$$

$$y_{i,t+1} = h_{i,t+1} + \varepsilon_{i,t+1}\,, \tag{3}$$

where $h_{i,t} \sim N(\mu_h, \sigma_h^2)$ and $u_{i,t+1}$ and $\varepsilon_{i,t+1}$ are independent noise terms. The coefficient β captures the extent to which the parents' endowment h is inherited by their children. In this sense, β is the parameter governing the true rate of persistence of social status across generations. In contrast, Equation (3) determines how well this endowment is reflected in the observed outcome y. A larger variance in the noise term, σ_ε^2, is associated with a lower observability of the endowment h.

The intergenerational elasticity of outcome y estimated from equation (1) is:

$$E(\hat{b}) = \beta\, \frac{\sigma_h^2}{\sigma_h^2 + \sigma_\varepsilon^2} := \beta\, \theta,$$

[2] For a more thorough review, see Solon (1999), Corak (2006), and Black and Devereux (2011).

Table B.4: Persistence of social status in the literature.

Panel A: Estimates of b		
$\hat{b}$	y_t	Country & Source
0.31–0.41	Wealth	Agricultural societies (Borgerhoff Mulder et al. 2009)
0.48–0.59	Wealth	UK (Harbury and Hitchins 1979)
0.225	Wealth	Norway (adoptees) (Fagereng, Mogstad, and Ronning)
0.6	Earnings	USA (Mazumder 2005)
0.34	Earnings	USA (Chetty et al. 2014)†
0.47	Earnings	USA (Corak 2006)
0.19–0.26	Earnings	Sweden (Jantti et al. 2006)
0.11–0.16	Earnings	Norway (Jantti et al. 2006)
0.46	Education	USA (Hertz et al. 2007)
0.71	Education	UK (Hertz et al. 2007)
0.35	Education	Sweden (Lindahl et al. 2015)
0.35	Body Mass	USA (Classen 2010)
Panel B: Estimates of β		
$\hat{\beta}$	y_t	Data & Source
0.70–0.75	Wealth	UK probate (1858–2012) (Clark and Cummins 2015)
0.70–0.90	Oxbridge	UK (1170–2012) (Clark and Cummins 2014)
0.61–0.65	Occupation	Germany, 3 gen. (Braun and Stuhler 2018)
0.49–0.70	Education	Germany, 4 gen. (Braun and Stuhler 2018)
0.6	Education	Spain, census (Güell, Rodríguez Mora, and Telmer 2015)
0.61	Schooling	Sweden, 4 gen. (Lindahl et al. 2015)
0.49	Earnings	Sweden, 4 gen. (Lindahl et al. 2015)
0.74	Education	EU-28, 3 gen. (Colagrossi, d'Hombres, and Schnepf 2019)
0.8	Education	Spain, census (Collado, Ortuno-Ortin, and Stuhler 2018)

† Rank-rank slope instead of log-log elasticity.

where $\theta < 1$ is an attenuation bias for β.

Several methods have been used to identify β. One is to exploit correlations in y across multiple generations.[3] According to the first-order Markov process described above, the elasticity of outcome y is $\beta\theta$ between parents, t, and children, $t+1$, and $\beta^2\theta$ between grandparents, t, and grandchildren, $t+2$ (as long as the signal-to-noise ratio is stable across generations). Hence, the ratio of these elasticities identifies β. Intuitively, β is identified because the endowment h is inherited, but the estimation bias θ is not—it is the same across two or three generations. Another identification strategy for β is to estimate intergener-

[3] Lindahl et al. (2015), Braun and Stuhler (2018), Colagrossi, d'Hombres, and Schnepf (2019).

ational regressions of equation (1)'s form with group-average data for siblings (Braun and Stuhler 2018) or for people sharing rare surnames (Clark and Cummins 2015). By grouping individuals with similar inherited endowments, the noise term ε is averaged away. Güell, Rodríguez Mora, and Telmer (2015) propose to identify β through the informational content of rare surnames (ICS)—a moment capturing how much individual surnames explain the total variance of individual outcomes.[4] This method only requires cross-sectional data, i.e., it does not require linking data across generations. Similarly, Collado, Ortuno-Ortin, and Stuhler (2018) estimate β using horizontal kinship correlations in the cross-section.

Panel B of Table B.4 reports estimates of β from these different approaches. The estimates range between 0.49 and 0.90, and hence are substantially larger than the parent-child elasticities b. Furthermore, Clark (2015)'s comprehensive evidence suggests that β is close to a "universal constant" across societies and historical periods. This finding is disputed by studies using the ICS (Güell et al. 2018) or multi-generation links (Lindahl et al. 2015; Braun and Stuhler 2018; Colagrossi, d'Hombres, and Schnepf 2019) instead of surname-averages.

In light of this evidence, the unobserved endowment that children inherit from their parents has often been interpreted as skills, preferences, or even genes. First, because these endowments reflect well the measurement error problem described here: wealth, income, education, etc. only reflect skills and innate abilities with noise. Second, because if β is a universal constant, it should reflect nature rather than nurture. In other words, if β does not vary substantially across time and space, an obvious conclusion is that institutions, social policies, or processes of structural economic transformation cannot affect social mobility in the long run.

We argue that these estimates may be subject to another source of bias in settings where favouritism or nepotism are prevalent. That is, where those with power and influence give preference to friends and relatives ahead of better-qualified outsiders. For example, estimates of occupational or wage persistence may be affected by the fact that certain jobs have higher entry barriers for outsiders than for sons of insiders. Econometrically, this introduces a different bias: selection.

Selection bias. Beyond measurement error, parent-child elasticities may be subject to sample selection: whether observations are sampled or not may be correlated with the unobserved endowment h inherited by children. This additional source of bias is inherent to data used to evaluate social mobility. It is present in applications that focus on a subgroup of the population, e.g., an occupation or those leaving wills. Specifically, in certain occupations relatives of insiders may be more likely to be observed. This kind of selection bias is typically addressed using natural experiments. Similarly, wealth elasticities rely on wills and probate records, where only those leaving wealth above a legal requirement are sampled (Clark and Cummins 2015). This sampling criterion is likely to be correlated with h, an individual's inherited endowments (e.g., social competence, skills, genes). Sample selection may also arise in applications covering

[4] The ICS is the difference in the R^2 of regressing y on a vector of dummies indicating surnames vs. a regression in which this vector indicates "fake" surnames.

the entire population. In census data linking several generations, families are not observed if a generation migrates or dies before outcomes are realized (e.g., wage, occupation choice). This attrition can be correlated with the underlying endowment h. Finally, life-history data collected retrospectively may suffer from recall bias. This bias depends on h if families with large endowments have better knowledge of their ancestors.

To see how selection affects intergenerational elasticity estimates, let s be a selection indicator such that $s_i = 1$ if family i is used in the estimation, and $s_i = 0$ if it is not. The intergenerational elasticity of y estimated from equation (1) is:

$$E(\hat{b}) = b + \frac{\text{Cov}\left(s_i y_{i,t},\ s_i e_{i,t+1}\right)}{\text{Var}\left(s_i y_{i,t}\right)}.$$

Note that if $\text{Cov}\left(s_i y_{i,t},\ s_i e_{i,t+1}\right) = 0$, then $\hat{b}$ is an unbiased estimate of b and a biased estimate of β due to measurement error, i.e., $\hat{b} = \theta\beta$. If the selection indicator, s_i, is correlated with the underlying endowments transmitted across generations, $h_{i,t}$ and $h_{i,t+1}$, then the condition above is violated and $\hat{b}$ is a biased estimate of b.

These two biases are fundamentally different. As described above, measurement error can be corrected using multiple generations. The reason is that across n generations, the underlying endowment is inherited $n-1$ times at a rate β but only twice transformed into the observed outcome y with measurement error. This is not true for the selection bias, which depends on the h, and hence is 'inherited' $n-1$ times. For example, consider grandparent-grandchild (and parent-child) correlations in outcomes: The correlations depend on β—which is inherited twice (once), on the measurement error with which h is twice (twice) transformed into y, and on the selection bias—which is also 'inherited' twice (once). Hence, the ratio of grandparent-grandchild to parent-child correlations does not correct for selection. Moreover, if selection changes over time (e.g., due to changes in the prevalence of nepotism) this bias may differ across two and three generations. In other words, the ratio of grandparent-grandchild to parent-child correlations may provide upward or downward biased estimates of β.[5]

Henceforth, we restrict our analysis to sample selection—the bias emerging when inherited human capital is correlated to whether families are sampled or not. Another issue is whether human capital endowments (h) are genetically inherited (selection) or are determined by parental investments (causation).[6] We abstract from this selection story as our main purpose is to disentangle nepotism from human capital endowments, regardless of whether the latter are determined by nature or nurture. That said, in our empirical application it is possible that a scholar strategically invests in the human capital of his most

[5] Formally, this ratio is an upward biased estimate of β if $\dfrac{\text{Cov}\left(s_i\, y_{i,t},\ s_i\, e_{i,t+2}\right)}{\text{Cov}\left(s_i\, y_{i,t},\ s_i\, e_{i,t+1}\right)} > 1$.

[6] Different strategies have been used to address this kind of selection: twin studies (Behrman and Rosenzweig 2002), adoptees (Plug 2004; Björklund, Lindahl, and Plug 2006; Sacerdote 2007; Majlesi et al. 2019; Fagereng, Mogstad, and Ronning), and policy changes affecting parents' outcomes exogenously (Black, Devereux, and Salvanes 2005). See Holmlund, Lindahl, and Plug (2011) and Black and Devereux (2011) for reviews.

endowed son, i.e., the son with higher chances of becoming a scholar *ex ante*. Unfortunately, we only observe the children of scholars who become scholars themselves. Hence, we cannot use sibling comparisons to address this issue. That said, such strategic investments in the most endowed son would lead to understating the rate of mean reversion in scholars' human capital and to overstating nepotism—which we already estimate to be low in periods of rapid scientific advancement.

C Identification

This appendix describes in more detail how our 13 moments identify the model's parameters and illustrates our identification strategy with simulations.

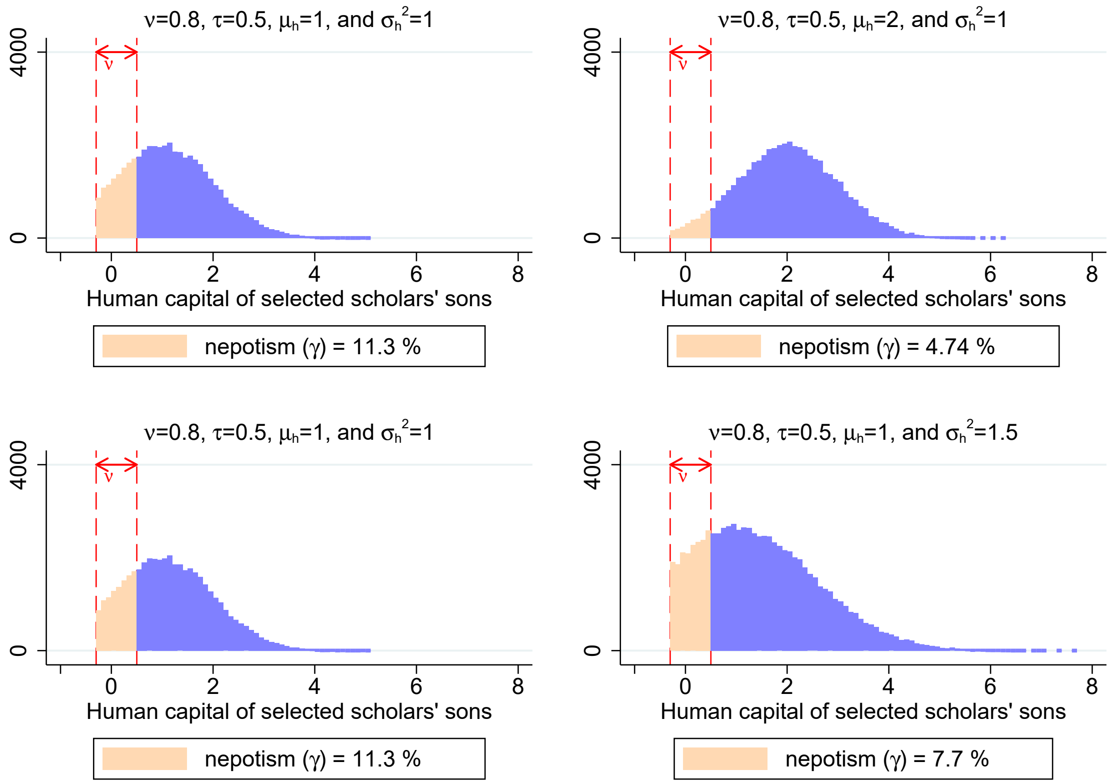
We identify the deep parameters of our model of human capital transmission with nepotism using the two Facts described in Section 3, Table 2. Specifically, we identify the intergenerational elasticity of human capital (β), the magnitude of nepotism (γ), the noise with which unobserved human capital is transformed into observed publications (σ_e and κ), and the shape of the human capital distribution (μ_h and σ_h) by minimizing the distance between 13 simulated and empirical moments in Table 2.[7] The 13 empirical moments used in the estimation can be grouped into two categories: First, as is standard in the literature, we consider correlations in observed outcomes across generations. Specifically, we consider the father-son correlation in publications conditional on both having at least one observed publication (intensive margin) and the proportion of families where father and son have zero publications (extensive margin). When observed, we also consider the grandfather-grandson correlation in the intensive margin. Second, we depart from the previous literature and consider ten moments describing the empirical distribution of publications for fathers and sons. These moments are the mean, the median, the 75th and 95th percentiles, and the proportion of zeros.

Before describing how the empirical moments used in the estimation identify the model's parameters, it is worth noting how γ, the magnitude of nepotism, depends on the other model's parameters. Specifically, γ is determined by parameters ν and τ, but also by the distribution of human capital among all potential scholars. In other words, $\tau - \nu$ alone does not characterize the magnitude of nepotism. For example, the same $\tau - \nu$ can reflect low levels of nepotism if the mean μ_h and the variance σ_h^2 of the stationary human capital distribution are high, and high levels of nepotism if μ_h and σ_h^2 are low. This is illustrated in Figure C.5, which shows the simulated distribution of human capital of sons of scholars under different model's parameters. All panels consider the same $\tau - \nu = 0.3$, but a different mean, μ_h, and variance, σ_h^2, for the human capital distribution. Specifically, the left panels consider a benchmarck scenario with $\mu_h = 1$ and $\sigma_h^2 = 1$. The top right panel considers a scenario with a larger mean ($\mu_h = 2$ and $\sigma_h^2 = 1$), and the bottom right panel a scenario with a larger variance ($\mu_h = 1$ and $\sigma_h^2 = 1.5$). Although ν

[7] The parameters μ_u and σ_u are pinned down from the stationarity conditions (6) and (7). We assume $\tau = 0$ without loss of generality and recover ν from equation (9).

is constant across panels, the share of nepotic sons varies considerably.

FIGURE C.5: The magnitude of nepotism (γ) and other model's parameters



Notes: Based on 50,000 simulated families of potential scholars.

Next, we describe how our 13 empirical moments identify the model's parameters. Father-son correlations provide biased estimates of β due to measurement error, governed by σ_e and κ, and due to selection from nepotism, γ. We address both biases by comparing not only observed *outcomes* across generations, but also the corresponding *distributions*. These comparisons respond differently to measurement error and nepotism, and hence can be used to identify the model's parameters.

In terms of observed *outcomes*, an increase in measurement error reduces the extent to which father-son correlations reflect β. The reason is that measurement error alters these correlations but not the underlying human capital endowments. In contrast, an increase in nepotism alters the human capital distributions for selected fathers and sons, and also the corresponding father-son correlations. Hence, these correlations may become more informative of β.

In terms of observed *distributions*, nepotism and measurement error also have different implications. Measurement error is not associated with differences in the distribution of the observed outcome y across generations. In contrast, nepotism lowers the selected sons' human capital relative to that of their fathers.

This generates distributional differences across generations (beyond those generated by reversion to the mean), as suggested by Figure 4. Intuitively, the distributional differences generated by nepotism are stronger at the bottom of the distribution, i.e., closer to the selection thresholds. Our estimation strategy, hence, puts additional weight on the proportion of father's and sons with zero publications. In addition, the variance of the distributions—captured by the 75th and 95th percentiles—also helps to disentangle measurement error from nepotism: an increase in measurement error increases the variance of both distributions, while an increase in nepotism increases the variance of the sons' distribution relatively more. In theory, this allows to correct for measurement error without resorting to grandfather-grandson correlations. That said, in our empirical application measurement error is governed by two parameters, σ_e and κ. This additional moment, i.e. grandfather-grandson correlations, helps to identify σ_e and κ separately.[8]

In sum, our identification strategy exploits the fact that an increase in the degree of nepotism (measurement error):

(i) generates (does not generate) father-son distributional differences;

(ii) increases (does not increase) the variance of sons' outcomes vs. their fathers';

(iii) increases (reduces) the information that father-son correlations convey about intergenerational human capital transmission.

Hence, by comparing both outcomes and distributions across generations, we can disentangle measurement error from selection and identify our model's parameters.

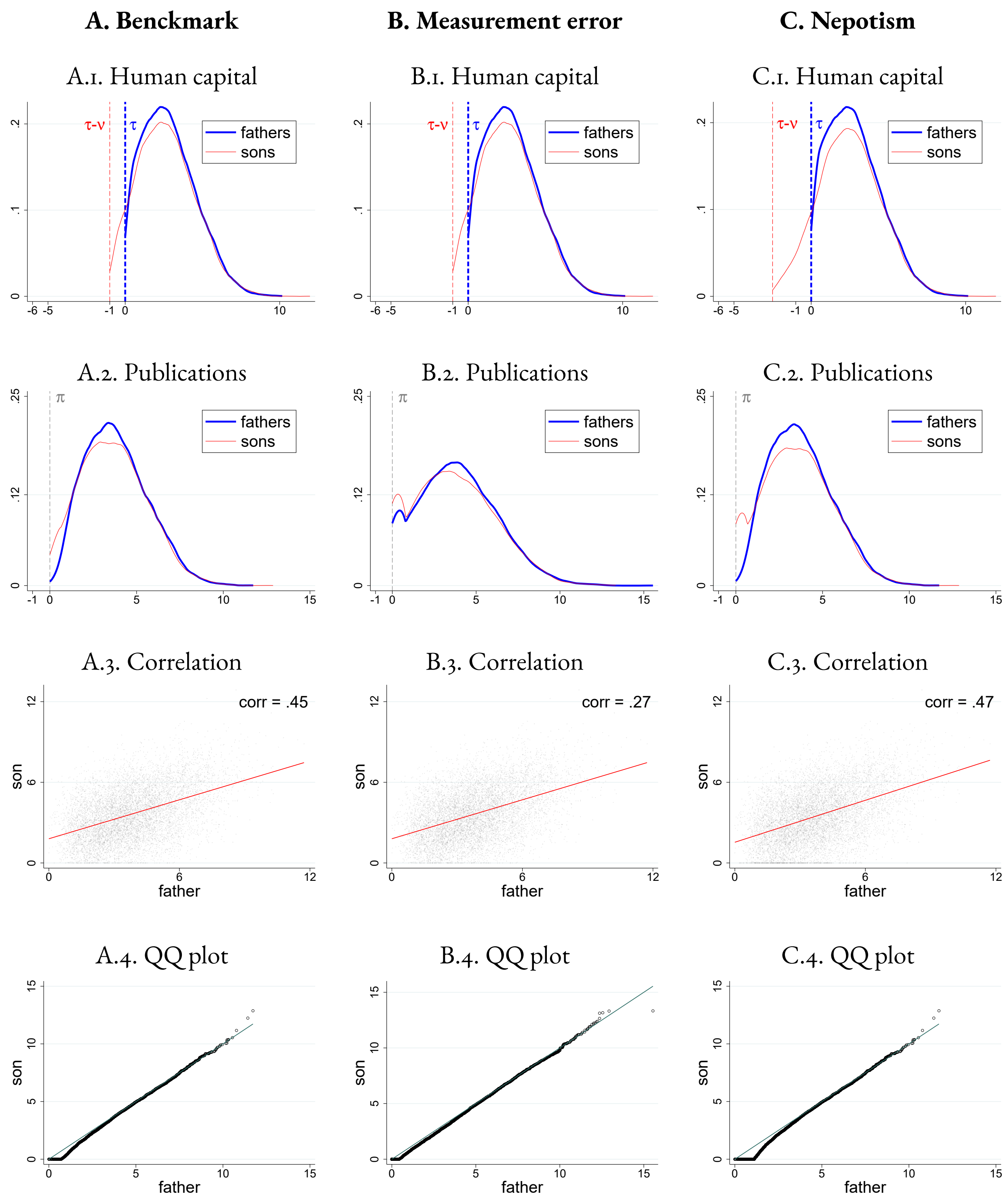
We illustrate our identification strategy with simulations. Figure C.6 shows the simulated distributions of the underlying (human capital) and the observed outcome (publications), father-son correlations in publications and the corresponding QQ plot. Column A presents a benchmark simulation for 10,000 potential scholars with $\beta = 0.6$, $\gamma = 13.5\%$, $\mu_e = 1$, $\pi = 0$, $\mu_h = 2$, $\sigma_h = 5$, $\sigma_e = 0.25$, $\tau = 0$, and $\nu = -1$. In Column B, we increase σ_e^2 to 3. That is, we generate measurement error by reducing the extent to which human capital translates into publications. The distribution of h is not altered with respect to the benchmark case, but that of y is: both fathers and sons present a larger mass of zero publications and a larger variance. Since y is similarly affected for fathers and sons, the QQ plot does not reflect distributional differences across generations. However, the increase in measurement error attenuates the father-son correlation in y, which drops from 0.45 to 0.27 with respect to the benchmark case.

Column C increases nepotism with respect to the benchmark case by setting $\gamma = 40\%$ (or, alternatively, by setting $\nu = -2.5$ with the remaining model parameters being constant). In contrast to the previous exercise, this affects the distribution of both h and y, as sons with low levels of human capital now can become a scholar.[9] This generates distributional differences in observed publications between

[8] In other words, for datasets in which κ is not binding, the measurement error bias is governed by one parameter, σ_e. This can be identified with the variance of the observed outcome's distribution across generations, without resorting to grandfather-grandson correlations.

[9] The father's h distribution is also affected, albeit to a lesser degree. The reason is that marginal fathers, i.e., fathers with an h just above the threshold τ, are now more likely to be in the set of selected families. Before, these fathers were mostly excluded,

Figure C.6: Identification example based on model simulations



Notes: The benchmark simulation is for 10,000 potential scholars with $\beta = 0.6$, $\gamma = 13.5\%$, $\mu_e = 1$, $\pi = 0$, $\mu_h = 2$, $\sigma_h = 5$, $\sigma_e = 0.25$, $\tau = 0$, and $\nu = -1$. Column B increases σ_e to 3, Column C increases nepotism by setting $\gamma = 40.2\%$.

as their sons were likely to have low realizations of h, falling below the (nepotic) threshold to become a scholar. Similarly, this may decrease the variance of fathers' publications.

fathers and sons, reflected in the QQ plot. Most evidently, the mass of sons with zero publications and the variance of sons' publications is now larger than their fathers'. Since nepotism alters both the human capital's and the observed outcome's distribution, father-son correlations become more informative of β than in the benchmark case: the correlation increases from 0.45 to 0.47.

In sum, measurement error and nepotism have different implications for father-son correlations, distributional differences (especially, at the bottom of the distribution), and the relative variances of observed outcomes.

D Model fit

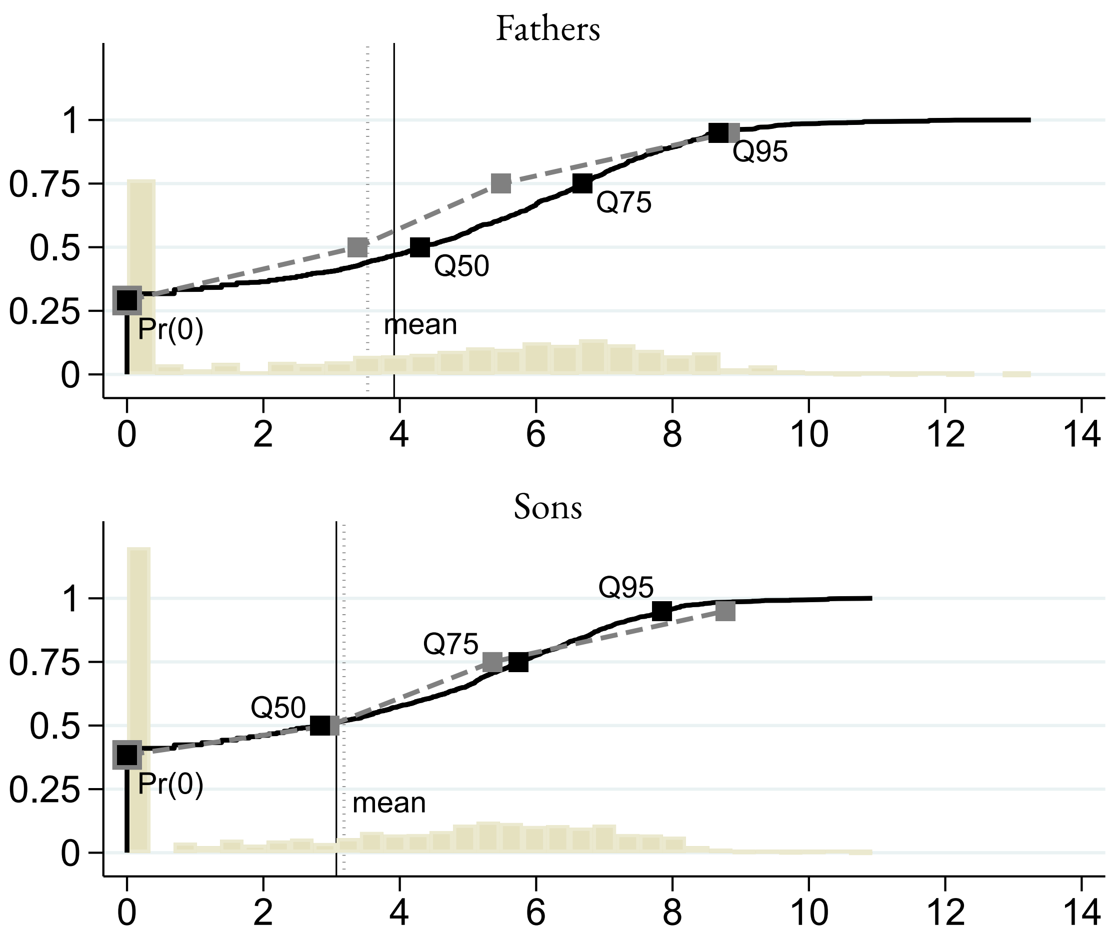
This appendix provides additional descriptive statistics and a detailed discussion on model fit, which is briefly summarized in the main text (see Section 5).

Our estimates reproduce Fact 2, that is, that the publications' distribution of fathers first-order stochastically dominates that of sons. Figure D.7 shows the distributional differences between fathers and sons. We plot the histogram for the logarithm of 1 + publications, the empirical cdf, and the simulated mean, median, 75th and 95th percentile, and the proportion of zeros. We fit both distributions: we perfectly match the proportion of fathers and sons with zero publications—the two moments to which our objective function attaches additional weight. We also match their means, medians, 75th and 95th percentiles. For fathers, we underestimate the number of publications, especially in the 75th percentile. Importantly, we reproduce Fact 2: The fathers' simulated distribution first order stochastically dominates that of sons. We match the fact that fewer fathers have zero publications, that fathers on average published more than sons, and that the median father, the father on the 75th, and on the 95th percentile published more than the corresponding sons. We also reproduce the empirical observation that the gap between fathers' and sons' publications is more prominent at the bottom of the distribution: our simulated moments reflect larger father-son gaps in the proportion of zero publications, the mean, and the median than in the 75th and 95th percentile. For example, the gap between fathers and sons (in levels) in the median is three times larger than in the 75th percentile.

As explained in the main text, nepotism is crucial for reproducing the father-son distributional differences. Specifically, we estimate an alternative model without nepotism, $\gamma = 0$. This model, where distributional differences can only be generated by mean reversion, fails to match Fact 2. In addition to the simulated and empirical data moments discussed in the main text, Table D.5 provides the estimated parameters the alternative model without nepotism (col. 1) and for our baseline model (col. 2).

Our estimates also reproduce Fact 1. The bottom rows of Table D.5 compare the simulated and empirical moments regarding correlations across generations. Our model with nepotism matches the father-son correlation on the intensive margin of publications – that is, conditional on both father and son having at least one observed publication. This is the correlation to which our objective function attaches addi-

Figure D.7: Publication's distribution, lineages of scholars



Notes: This figure displays the histogram and the cdf of fathers' and sons' publications. Data (black), simulated moments (grey), and moments (labels).

tional weight. Interestingly, this correlation is below the estimate of β. This implies that father-son correlations in outcomes under-predicts the extent to which children inherit human capital endowments from their parents. Our model with nepotism under-predicts the proportion of families where father and son have zero publications (extensive margin) and the correlation between grandfathers and grandsons in the intensive margin. That said, we match the empirical fact that the grandfather-grandson correlation is larger than predicted by iterating the two-generation correlation. Specifically, our simulated grandfather-grandson correlation is 0.19. In contrast, iterating the simulated two-generation correlation yields $0.37^2 = 0.1369$.

TABLE D.5: Simulated and empirical moments for different models.

	Model w/o nepotism	Baseline model	Data
Parameters:			
β	0.66	0.63	.
γ	0 *(imposed)*	18.8	.
μ_h	4.20	1.72	.
σ_h	2.27	3.88	.
σ_e	0.80	0.35	.
κ	3.54	1.97	.
Moments:			
Fathers with zero publications	0.36	0.29	0.29
Sons with zero publications	0.36	0.38	0.38
Median, fathers	4.35	3.38	3.92
Median, sons	4.34	2.97	2.83
75th percentile, fathers	5.93	5.48	6.68
75th percentile, sons	5.93	5.36	5.74
95th percentile, fathers	8.24	8.84	8.66
95th percentile, sons	8.18	8.77	7.85
Mean, fathers	3.67	3.53	3.92
Mean, sons	3.65	3.18	3.07
Father-son correlation†	0.34	0.37	0.37
Father-son with zero publications	0.21	0.17	0.22
Grandfather-grandson correlation†	0.21	0.19	0.22

Notes: †correlation on the intensive margin.

E QQ plots

FIGURE E.8: Quantile-quantile plot by historical period

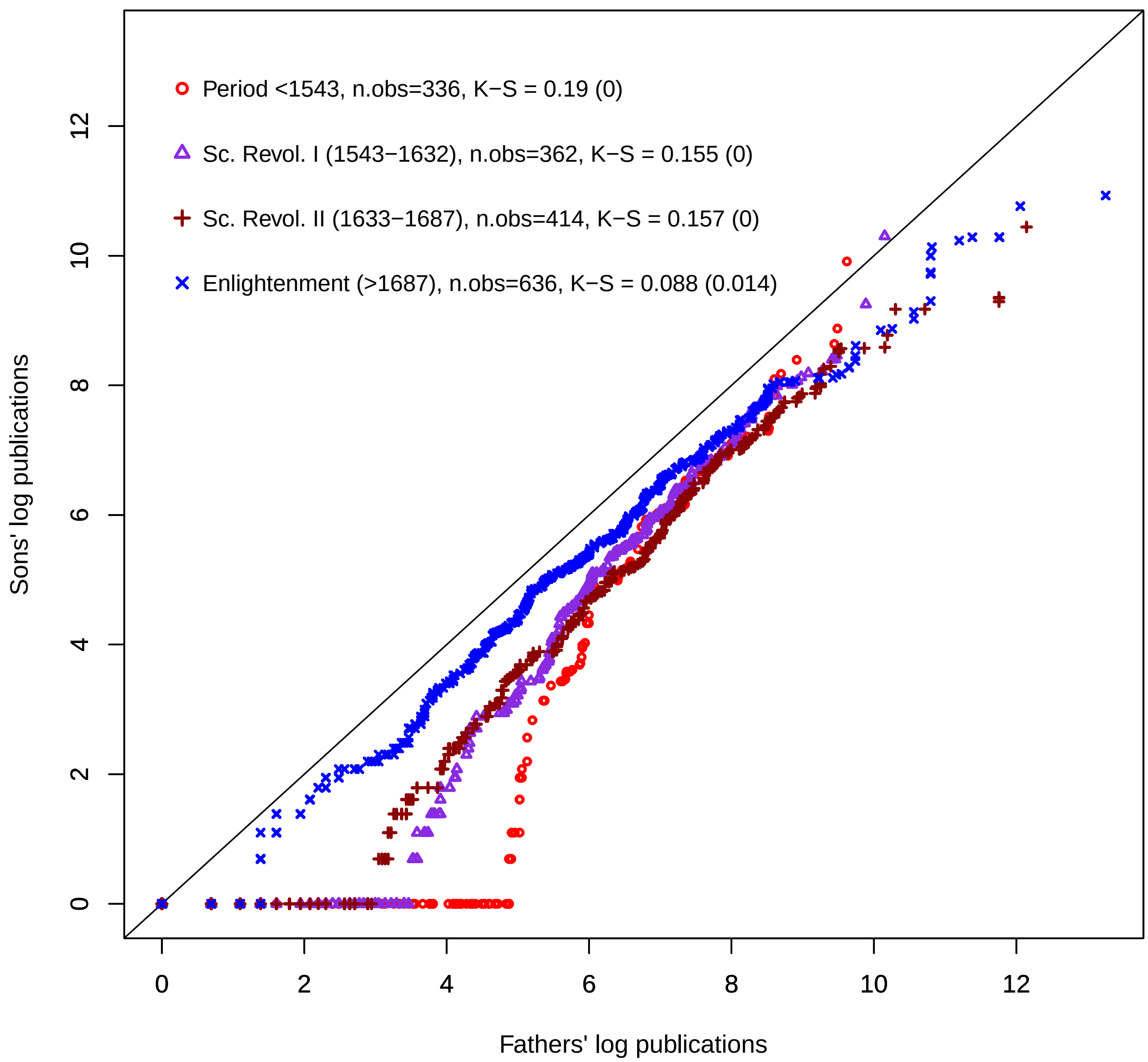


Figure E.9: Quantile-quantile plot by age of institution

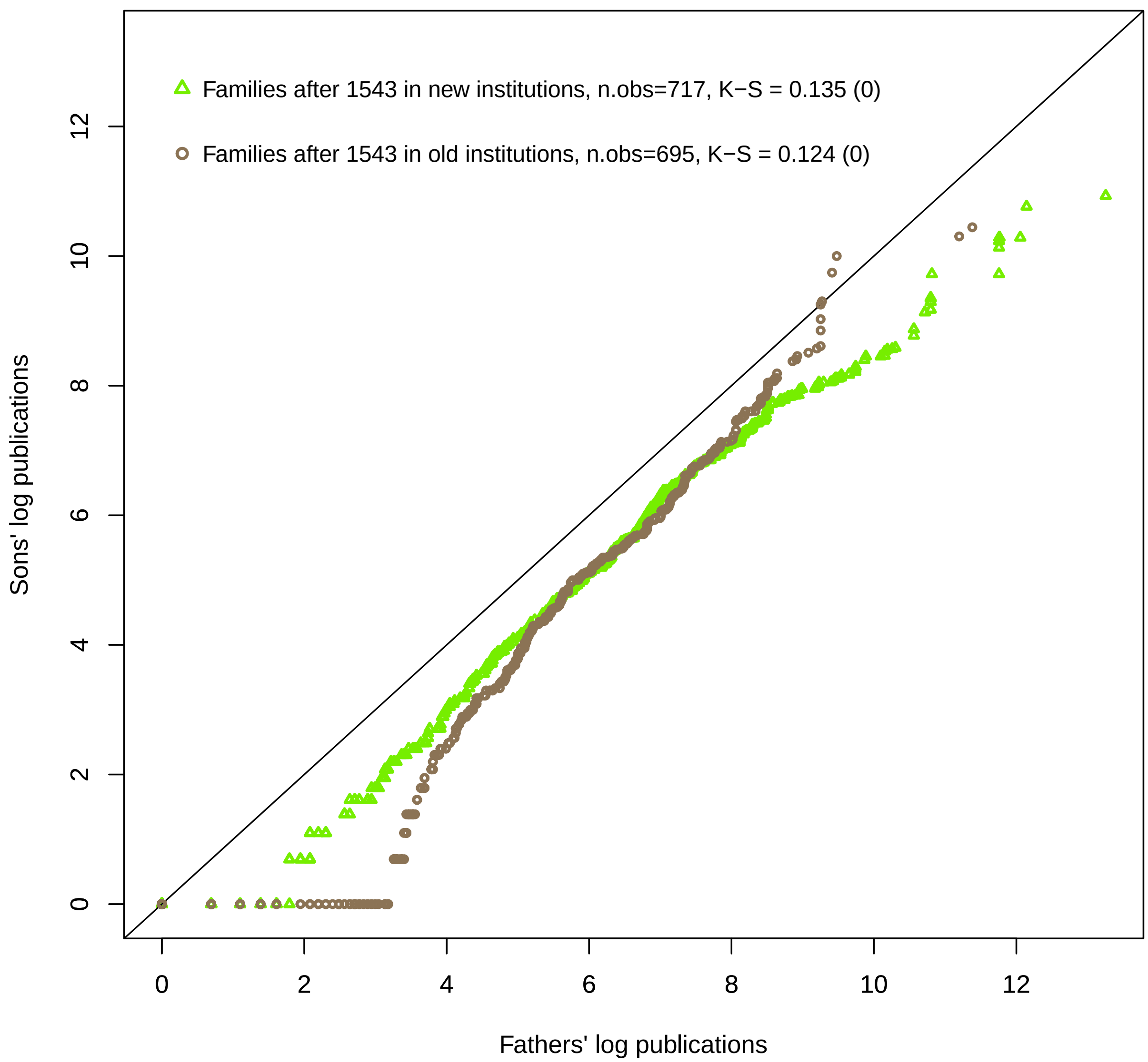


Figure E.10: Quantile-quantile plot by religious affiliation

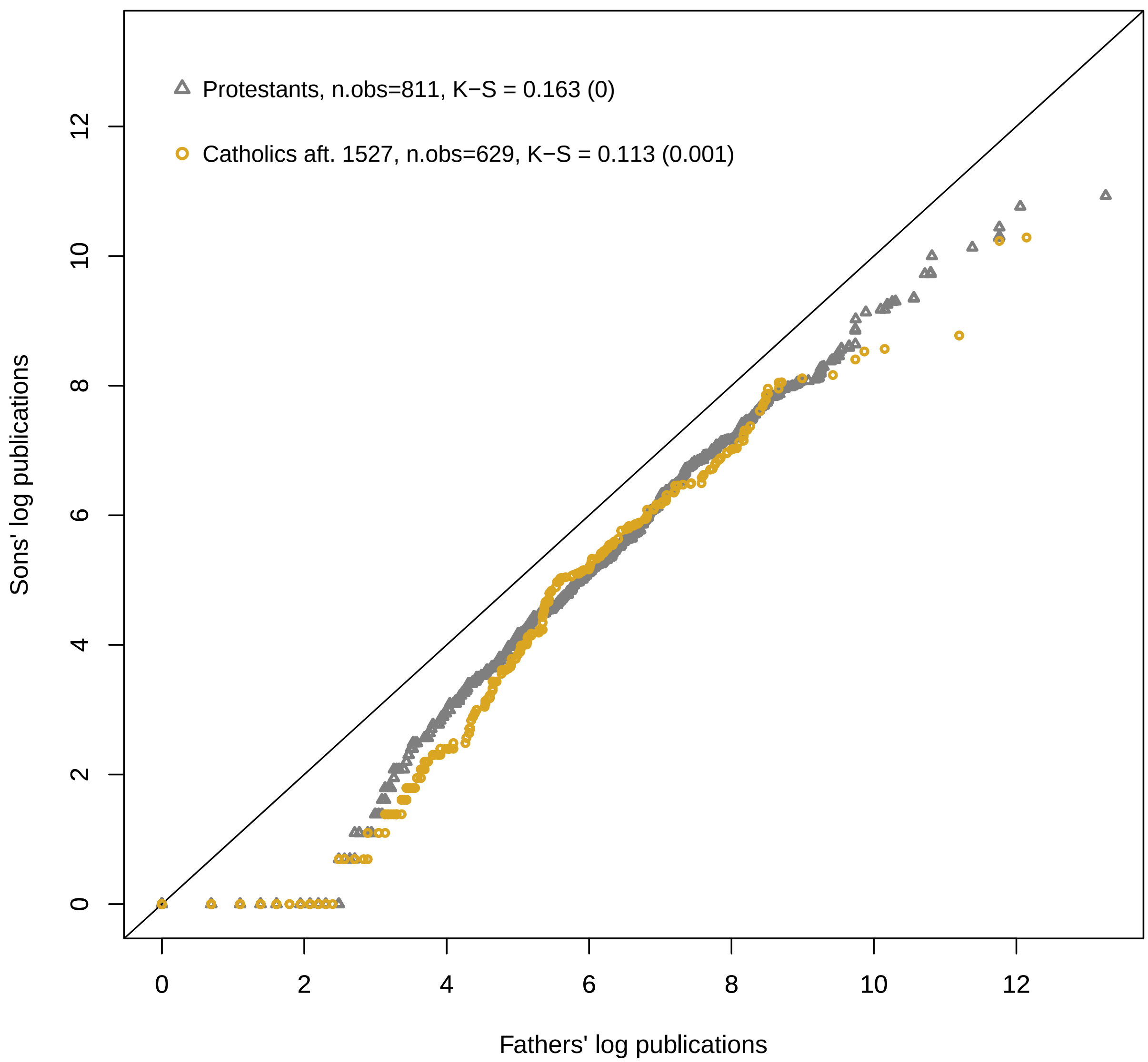


Figure E.11: Quantile-quantile plot by field of study

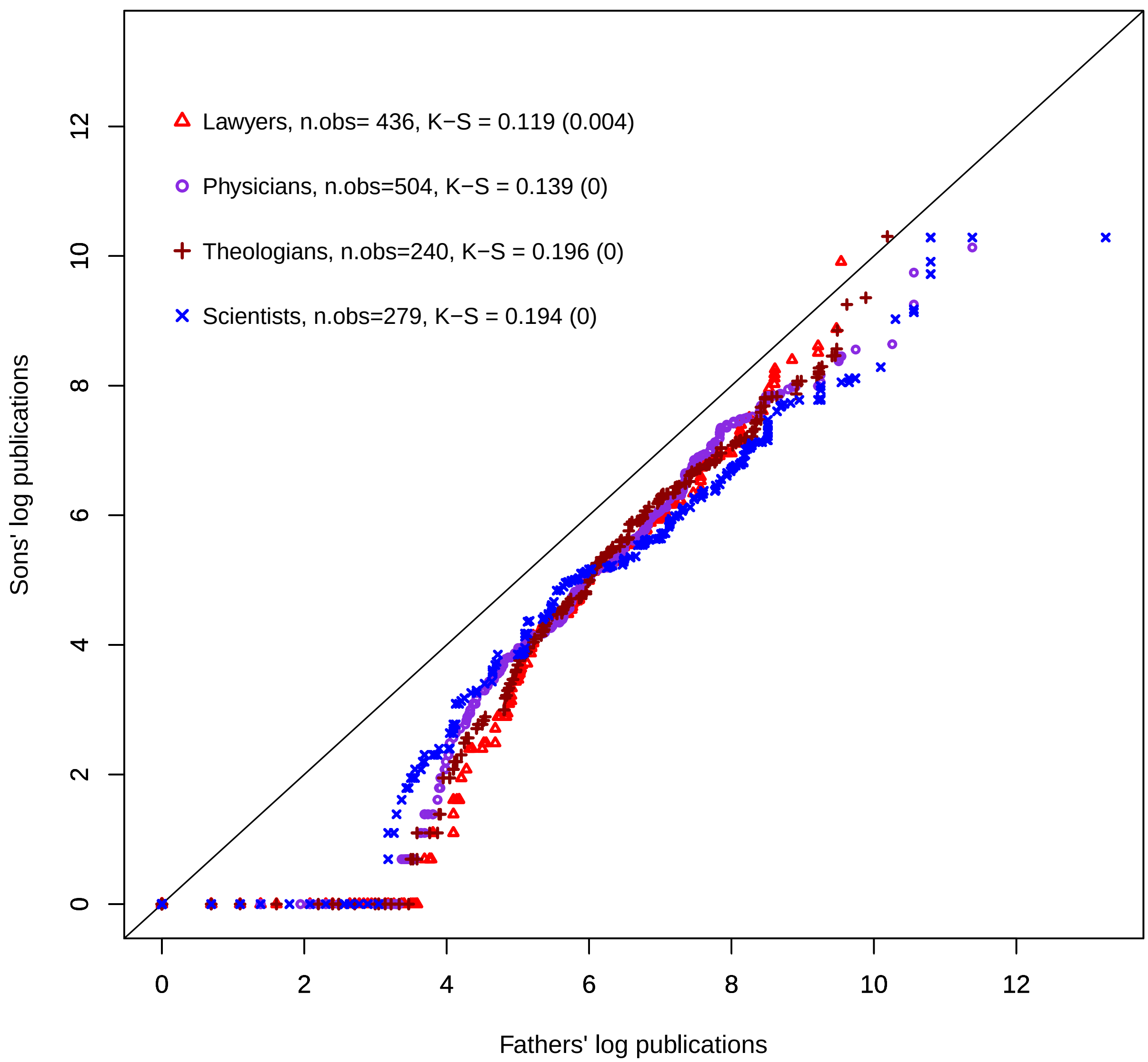


Figure E.12: Quantile-quantile plot by nomination bef./after father's death

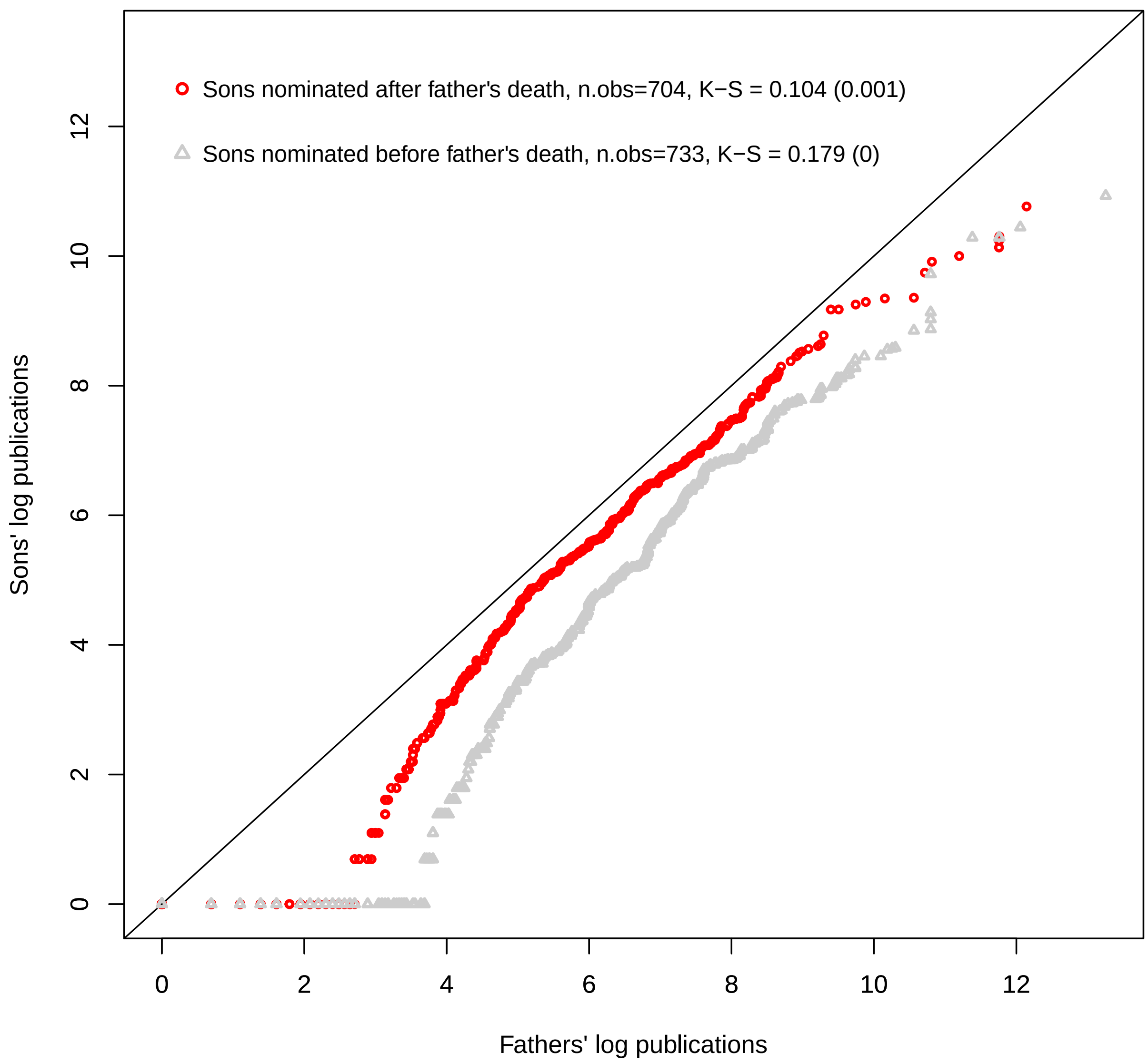
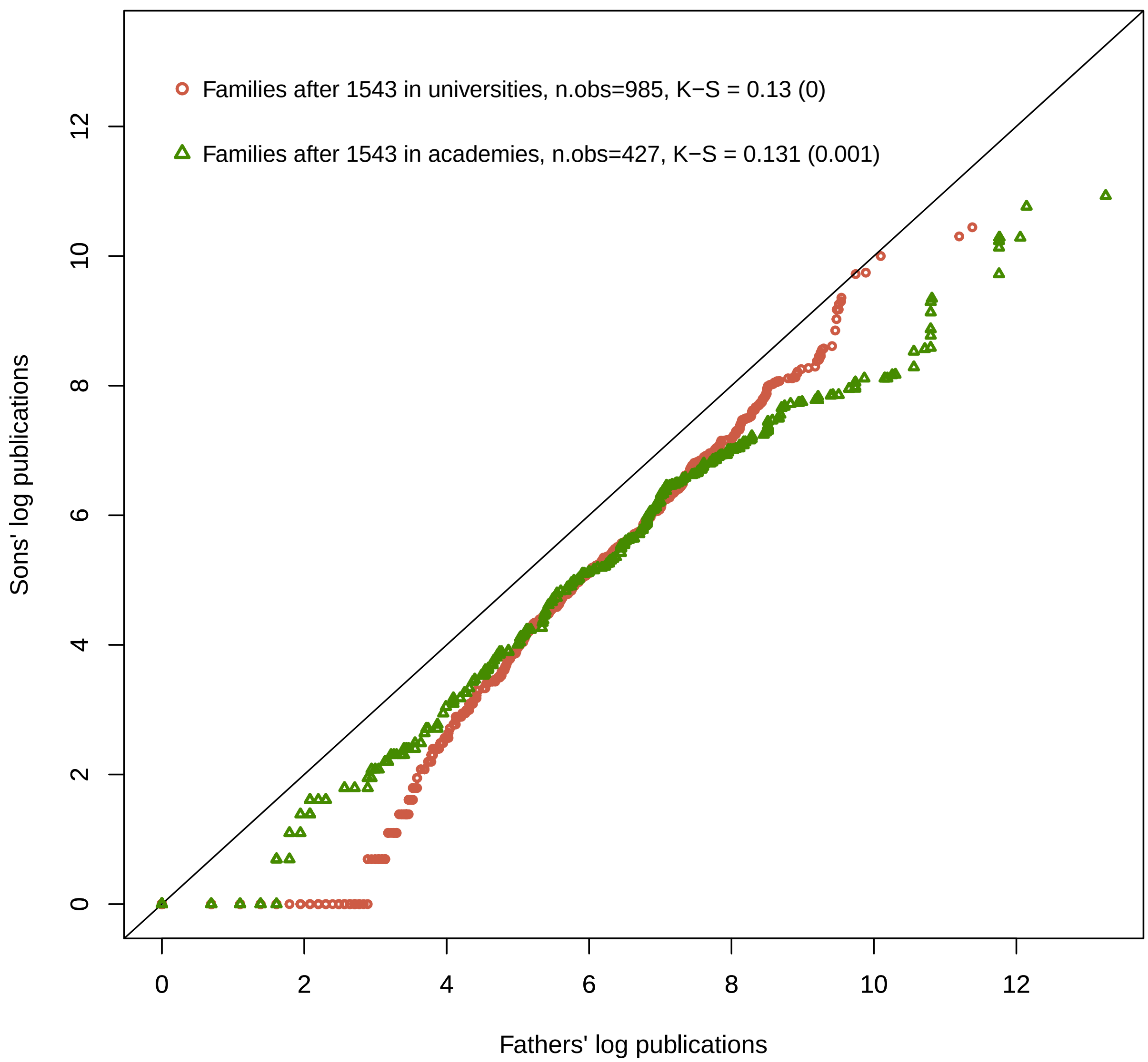


Figure E.13: Quantile-quantile plot by type of institutions



F Moments used in estimation with complete and complete & broad coverage

In the main text, we examine the sensitivity of our analysis to sampling bias. That is, to the possibility that the secondary sources used to construct our dataset selectively report father-son links when fathers are famous. In short, we show that this scenario is unlikely for four reasons: First, two thirds of our father-son pairs are based on sources with complete coverage where we can rule out sampling bias, and the remaining third comes mostly from sources whit broad coverage where sampling bias is unlikely. Second, the coverage of the data (complete, broad, or partial) does not vary substantially over the historical periods under analysis, over centuries, across countries, across fields of study, and by the religion of the university (protestant vs. catholic). Third, it is not obvious why secondary sources would selectively record famous fathers of underachieving son more often than underachieving fathers of famous sons. Fourth, we present separate estimates restricting the data to sources with complete coverage and to sources with complete and broad coverage, and show that our results are robust. In this appendix, we provide the detailed summary statistics of the moments used in the estimations with complete coverage and with complete and broad coverage. We show that the two Facts used in our estimation strategy are robust to the accuracy of our sources, and hence, are also not a by-product of sampling bias.

Specifically, Table F.6 presents the 13 moments used for our baseline estimation (column 1), for our estimation restricted to sources with complete coverage (column 2), and for our estimation restricted to sources with complete and broad coverage (column 3). Panel A shows the moments capturing intergenerational correlations. If the historical sources used are subject to sampling bias, we would expect our baseline intergenerational correlations to be downward biased relative to those calculated using sources where we can rule out sampling bias (Solon 1989). Instead, we find that the father-son elasticity of publications is 0.37 for all families, 0.34 for families with complete coverage only, and 0.37 for families with complete and broad coverage. Similarly, the grandfather-grandson elasticity of publications is, respectively, 0.22, 0.16, and 0.22. On the extensive margin, the proportion of fathers *and* sons with zero publications is around 0.20 for all families, families with complete coverage, and families with complete and broad coverage. This suggests that the relatively high elasticity of publications across generations (Fact 1) is not a by-product of sampling bias in our sources, as it is observed also in the subsample restricted to complete coverage where we observe the universe of father-son pairs.

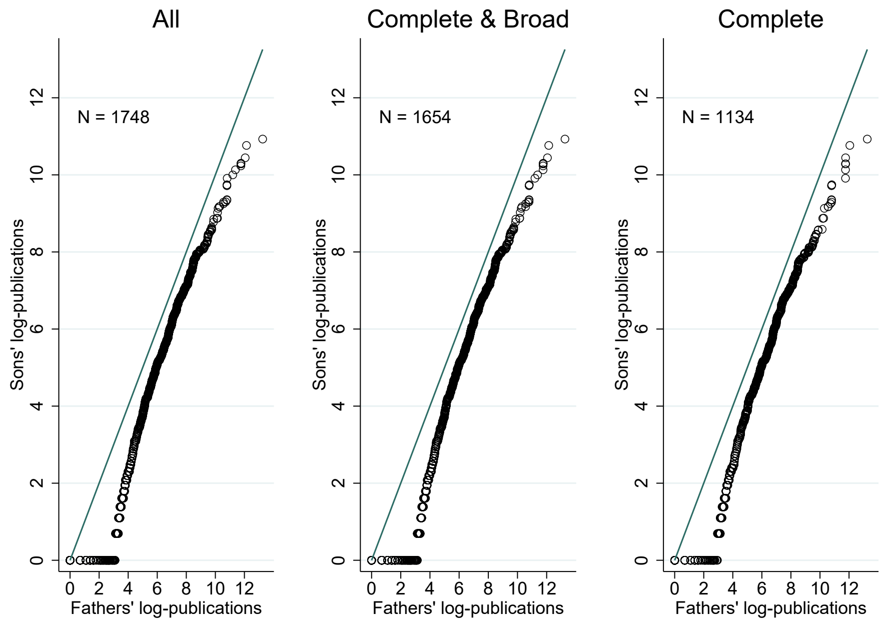
Panel B of Table F.6 shows the moments capturing father-son distributional differences. If fathers who were scholars of no great account are more likely to fall by the wayside than an underachieving son of a famous scholar, we would expect this sampling bias to drive the wedge between the fathers' and sons' publication distribution. In other words, we would expect our baseline distributional differences to be substantially larger than those calculated using sources where we can rule out sampling bias. Instead, we find that the distributional moments are very similar for all families, for families with complete coverage

Table F.6: Moments, by coverage of data sources.

		All [1]	Complete [2]	Complete and Broad [3]
A. Intergenerational correlations				
Father-son, intensive margin	$\rho(y_t, y_{t+1} \mid_{y>0})$	0.37	0.34	0.37
Father-son with zero publications	$\Pr(y_t{=}y_{t+1}{=}0)$	0.22	0.19	0.21
Grandfather-grandson, intensive margin	$\rho(y_t, y_{t+2} \mid_{y>0})$	0.22	0.16	0.22
B. Father-son distributional differences				
Fathers with zero publications	$\Pr(y_t{=}0)$	0.29	0.25	0.29
Sons with zero publications	$\Pr(y_{t+1}{=}0)$	0.38	0.34	0.38
Fathers median	$Q50(y_t)$	4.30	4.75	4.36
Sons median	$Q50(y_{t+1})$	2.83	3.64	2.97
Fathers 75th percentile	$Q75(y_t)$	6.68	6.83	6.68
Sons 75th percentile	$Q75(y_{t+1})$	5.74	6.03	5.78
Fathers 95th percentile	$Q95(y_t)$	8.66	8.92	8.68
Sons 95th percentile	$Q95(y_{t+1})$	7.85	7.94	7.85
Fathers mean	$E(y_t)$	3.92	4.21	3.96
Sons mean	$E(y_{t+1})$	3.07	3.38	3.11
Father-son pairs	N	1,748	1,134	1,654

only, and for families with complete and broad coverage. For example, the proportion of sons with zero publications is 9 percentage points larger than the proportion of fathers with zero publications in all three groups. The median, 75^{th} and 95^{th} percentile, and mean are also larger for fathers than for sons across groups. To illustrate the similarity of the father-son distributional differences, Figure F.14 presents QQ plots for each group. The fathers' distribution of publications first order stochastically dominates that of sons independently of the coverage of the sources. Altogether, this shows that the distributional differences between fathers and sons (Fact 2) holds when we restrict our data to complete sources where we can rule out sampling bias. In other words, it is highly unlikely that the data is selected on father's publications and that this drives the observed wedge between the publications of fathers vs. sons.

FIGURE F.14: Quantile-quantile, by data coverage



G Stationarity and time trends in publications

To estimate nepotism and the intergenerational human capital elasticity, we assume that the human capital distribution is stationary among *potential scholars*. That is, among individuals with high human capital endowments who could potentially become scholars—whether they are in our dataset or not. This assumption is standard in the literature estimating intergenerational elasticities, but its importance is rarely discussed (Nybom and Stuhler 2019). In this appendix, we first discuss the use of the stationarity assumption in the literature and the sensitivity of our β-estimates to it. Next, we show that, under stationarity, our (already large) nepotism estimates are a lower-bound to the true level of nepotism. Finally, we use a dataset on all pre-modern scholars (not only fathers and sons) collected by de la Croix (2021) to examine time trends in observed outcomes. These trends support the stationarity assumptions for both our nepotism and β-estimates. In addition, Section 5.3 of the main text relaxes the stationarity assumption. Specifically, we assume that the human capital of a father and a son who were active in a given time period is drawn from the same distribution, but we allow the human capital distribution to change across periods. This allows publications to exhibit time trends on both the extensive or intensive margin.

G.1 Stationarity in the intergenerational literature

Theory. Steady-state assumptions play a critical role for intergenerational elasticities, especially when the endowments that parents transmit to children are unobserved.[10] To see this, consider the following

[10] See, e.g., Clark and Cummins (2015), Adermon, Lindahl, and Waldenström (2018).

first-order Markov process:

$$\begin{aligned} h_{i,t+1} &= \beta h_{i,t} + u_{i,t+1} \,, \\ y_{i,t+1} &= h_{i,t+1} + \varepsilon_{i,t+1} \,, \end{aligned}$$

where $h_{i,t} \sim N(\mu_{h,t}, \sigma^2_{h,t})$ is an unobserved endowment (human capital) that parents t transmit to children $t+1$ at rate β; y is an observed outcome (publications) noisily related to h; and $u_{i,t+1}$ and $\varepsilon_{i,t+1}$ are noise terms with standard deviation σ_u and σ_e. Here $\mu_{h,t}$ and $\sigma_{h,t}$ are time dependant. In other words, we do not impose stationarity over the human capital distribution.

We can estimate β using correlations in y across multiple generations.[11] The OLS elasticity of y between parents and children (b_1) and the corresponding elasticity between grandparents and grandchildren (b_2) are:

$$\begin{aligned} b_1 &= \beta \left[\sigma^2_{h,t+1} \,/\, (\sigma^2_{h,t+1} + \sigma^2_{\varepsilon})\right] , \\ b_2 &= \beta^2 \left[\sigma^2_{h,t+2} \,/\, (\sigma^2_{h,t+2} + \sigma^2_{\varepsilon})\right] , \end{aligned}$$

Hence, the ratio b_2/b_1 identifies β under the assumption that $\sigma_{h,t+1} = \sigma_{h,t+2}$. That is, when the signal-to-noise ratio is constant across three generations. This condition is satisfied when the human capital distribution is stationary. However, this stationarity assumption is often implicit, and its importance in estimating β is rarely acknowledged in the literature (Nybom and Stuhler 2019).

Evidence. Next, we present evidence supporting the stationarity assumption $\sigma_{h,t+1}=\sigma_{h,t+2}$. Ideally, we would show that the standard deviation of human capital h is constant over time for the universe of *potential scholars*. Since, by construction, we do not observe h, we will show trends in the standard error of the mean for our observed human-capital proxy: publications. To evaluate a universe resembling all *potential scholars*, we use the de la Croix (2021) data on 53,022 pre-modern scholars (not only fathers and sons) with a reference date in 1088–1800.[12]

Figure G.15 presents these trends, calculated over 25-year intervals. After 1350, the standard error of the mean of log-publications is extremely stable. This supports the assumption of a stable variance in the human capital distribution over time, that is, that $\sigma_{h,t+1} = \sigma_{h,t+2}$ is satisfied. Admittedly, the standard error is much larger before 1350. That said, in our dataset we have 34 families where both father and son's reference date is before 1350. Hence, it is unlikely that the large changes in standard error before 1350 are driving our aggregate β-estimates.

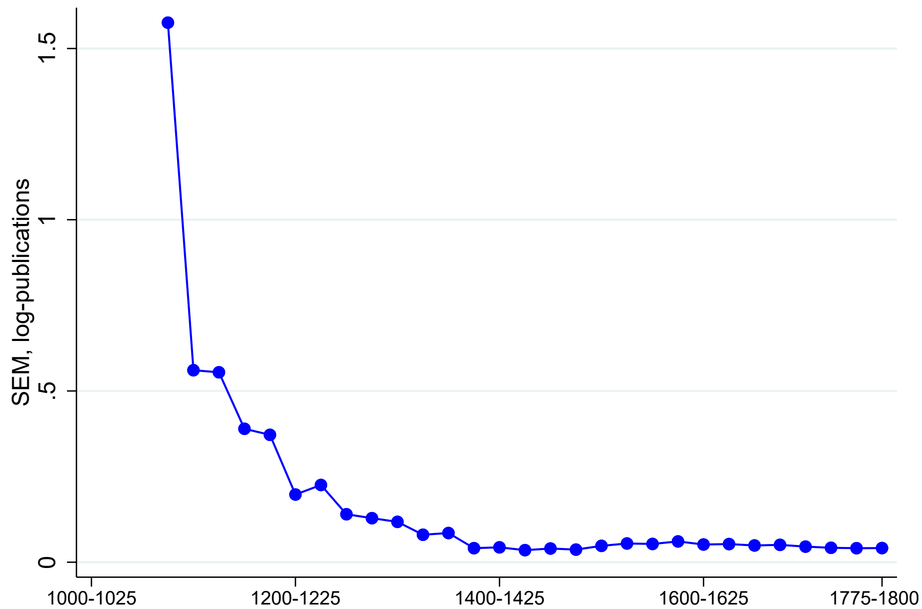
G.2 Stationarity and nepotism

Theory. Our estimates for nepotism are also sensitive to the stationarity assumption. Here we argue that, under stationarity, our nepotism estimates are lower-bound estimates. Note that we identify nepotism

[11] Lindahl et al. (2015), Braun and Stuhler (2018), Colagrossi, d'Hombres, and Schnepf (2019).

[12] Reference dates are based on birth year, nomination year, or approximate activity year.

Figure G.15: Trend in standard error of the mean, log-publications



Notes: The sample is all scholars in de la Croix (2021) with a reference date between 1088 and 1800. Standard error of the mean in log-publications calculated over 25-year periods.

using two sets of moments: The first are correlations in observed outcomes across multiple generations. These allow us to uncover the true intergenerational human capital elasticity, which will be important to estimate nepotism. The second are distributional differences in observed outcomes between fathers and sons. We argue that the observed distributional differences may be the result of two forces: on the one hand, nepotism lowers the selected sons' human capital relative to that of the selected fathers, generating distributional differences in publications. That said, not all the distributional differences are directly attributed to nepotism. The second force at place is mean-reversion. If human capital strongly reverts to the mean, the sons of individuals at the top of the human-capital distribution will perform worse than their fathers even if no nepotism is at place. To gauge how much do distributional differences depend on nepotism and how much on mean-reversion, we assume stationarity in the distribution of human capital over all potential scholars. The stationarity assumption and our first set of moments (which identify the intergenertional human capital elasticity β) allow us to uncover the rate of mean-reversion. That is, how different fathers and sons are supposed to look like in the absence of nepotism. Hence, any excess distributional differences, net of reversion to the mean, can be attributed to nepotism. Formally, imposing stationarity implies that the difference in human capital between fathers and sons should follow:

$$h_{i,t+1} = \beta h_{i,t} + (1-\beta)\mu_h + \omega_{i,t+1}\,,$$

where $\omega_{i,t+1}$ is a shock distributed according to $N(0, (1-\beta^2)\sigma_h^2)$. In the absence of nepotism, this differences in human capital would be directly translated into the following differences in publications:

$$y_{i,t} = \max(\kappa, h_{i,t} + \epsilon_{i,t})$$

$$y_{i,t+1} = \max(\kappa, \beta h_{i,t} + (1-\beta)\mu_h + \omega_{i,t+1} + \epsilon_{i,t+1})$$

If the father-son difference in publications is larger than suggested by the previous equations, then an additional force must be in place. A force selecting fathers and sons differently, such that the later can become scholars with lower human capital endowments. In our setting, this additional force is nepotism.

How would our nepotism estimate change in a non-stationary environment? In our setting, it is reasonable to assume that if the human capital distribution is non-stationary, then it *improves* over time. Under this scenario we would expect more sons with higher human capital than their fathers than under stationarity. This implies that, in the absence of nepotism, we would expect virtually no distributional differences in publications between fathers and sons. In extreme scenarios, we would even expect the sons publication's distribution to dominate that of their fathers. Hence, we would need a larger nepotism parameter to reconcile the large observed father-son distributional differences in publications with the small expected differences. In other words, under stationarity, a share of the father-son distributional differences is attributed to nepotism, and another to mean-reversion. In a non-stationary environment, mean-reversion would explain a lesser share of the father-son distributional differences, and hence, our nepotism estimate would have to be larger. Therefore, under stationarity, our nepotism estimates are conservative, lower-bound estimates.

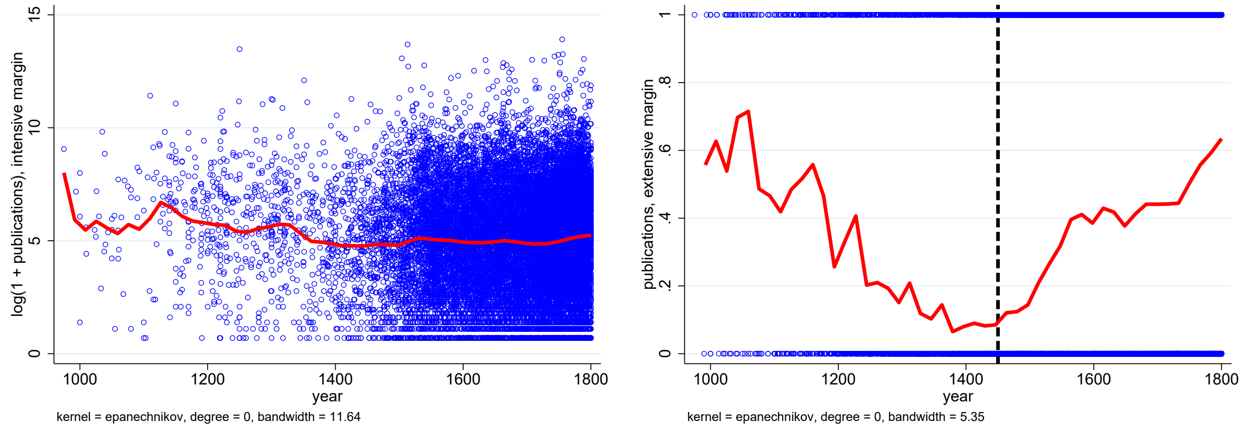
Evidence. The fact that our (already large) nepotism estimate is a conservative estimate is reassuring. Here we present additional evidence supporting the stationarity assumption, and hence, that our nepotism estimate is not severely downward biased. Ideally, we would show that the mean of the human capital distribution, μ_h, is constant over time for all *potential scholars*. Since we do not observe h, we will focus on trends in our observed human-capital proxy: publications. To evaluate a universe resembling all *potential scholars*, we use the dataset collected by de la Croix (2021) on 53,022 pre-industrial scholars.

Figure G.16 shows the trend in log-publications on the intensive margin (left panel). That is, conditional on having at least one publication listed in WorldCat. To calculate trends, we use a kernel-weighted local polynomial regression of publications on a scholar's reference date. The figure shows no trend in the intensive margin of publications, supporting our stationarity assumption.[13]

The right panel shows trends on the extensive margin of publications: that is, whether a scholar has at least one publication in WorldCat. The figure shows a U-shaped pattern. Before 1350, the extensive margin of publications is high because of a selection effect: top scholars are more likely to be observed. That said, we have a limited number of observations from this period (34), and hence, it is unlikely that

[13] The fluctuations before 1350 are driven by a smaller sample in the earlier periods.

FIGURE G.16: Trend in log-publications, intensive and extensive margin



Notes: The sample is all scholars in de la Croix (2021). Trend calculated with a kernel-weighted local polynomial regression. The dashed line is for the introduction of the printing press.

this has a large impact on our aggregate results. Around 1450, when the printing press was introduced, there is a structural break in the extensive margin of publications. There are two reasons to believe that this structural break does not reflect a change in the human capital distribution but a change in the technology for printing and preserving books: The first reason is that the printing press massively increased the diffusion and preservation of scholar's books (Dittmar 2019). This alone could explain the observed trend without resort to changes in the human capital distribution. Formally, this trend is related to our parameter κ, the measurement error on the extensive margin of publications, and not to μ_h, the mean of the human capital distribution among potential scholars. This is supported by our higher estimates for κ for the earlier period between 1088 and 1543 (see Section 5.3). The second reason why it is unlikely that this trend reflects changes in the human capital distribution is because such a change would affect the trends in *both* the extensive and the intensive margin of publications. We only observe a trend in the former, suggesting that the changes are related to improvements in the printing and book-preservation technology. Finally, this increasing trend implies that, around 1450, some sons benefited from the existence of the printing press to publish and preserve their work. In contrast, we are more likely to observe zero-publications for their fathers, whose work was not printed and may have been lost. Correcting for this bias would increase the father-son distributional differences. Hence, it would lead to larger nepotism estimates.

In sum, the de la Croix (2021) dataset comprising 53,022 scholars shows no trend on the intensive margin of publications. This supports our stationarity assumption for the human capital distribution. On the extensive margin, we find evidence of a structural break around 1450. That said, this is related to the changes brought about by the printing press in terms of book diffusion and preservation, rather than with a change in the human capital distribution.

H Robustness to distributional assumptions

The intergenerational transmission of wealth is often modeled assuming a normal distribution for the initial distribution of wealth $h_{i,t}$ and the idiosyncratic shock $u_{i,t+1}$. How do these distributional assumptions affect our results? Could the large nepotism estimate be a by-product of these distributional assumptions? Here we consider an alternative to normality: drawing shocks from fat-tailed distributions. This distributions give a higher likelihood to the emergence of geniuses, which is appealing in our setting with individuals at the very top of the talent distribution.

Before re-estimating our results, we need to consider two issues: the first concerns the targeted moments, the second the set of feasible fat-tailed distributions. Some of the commonly targeted moments when shocks are normal are not defined when shocks are fat tailed. This is the case of Pearson correlation and of the mean. Hence, if we want to use shocks from fat tailed distributions, we need to target an alternative set of moments ($V_S(p)$). Specifically, we replace the Pearson correlation for the Spearman rank correlation—which remains well-defined with any distribution—and we drop the two means from the targeted moments. We thus have four overidentifying restrictions instead of six. To show that these changes are not crucial for our results, we first conduct our baseline estimation under this new set of moments to define a new benchmark. Table H.7 presents the results and compares the to the estimation in the main text ($V_(p)$). First, note that the Spearman correlations ρ_S are similar to their Pearson counterparts ρ. Second, all estimates are similar under the two different objectives. In detail, our two main estimates—the intergenerational human capital elasticity, β, and the magnitude of nepotism, γ—are not significantly different when we target the moments in $V_(p)$ or in $V_S(p)$. Overall, the table shows that targeting this alternative set of moments does not alter our baseline results, and hence, that we can use them to check the robustness to using fat tailed shocks.

The second issue is the set of feasible fat tailed distributions. We need distributions with closed-form expressions for the density to verify that their shape is preserved (up to scale and shift) under addition. To see why, note that the sum-stable property of the normal distribution implies that its shape remains the same across all generations once transformed by Equation 2, that is, $h_{i,t+1} = \beta h_{i,t} + u_{i,t+1}$. Only its parameters change. This stability property is not a theoretical curiosity. Without it, we lack of coherence in modeling, as the initial distribution of human capital could not be rationalized by the model, its shape having vanished after one period. There are two families of fat-tailed distributions where one can verify that the sum-stable property is satisfied as in the normal distribution: The Cauchy and Levy distribution (Nolan 2003). Here we use the Cauchy distribution, which is fat tailed but, unlike the Levy distribution, is defined over $\mathbb{R}$.

We can now analyze the effect of using fat tailed distributions on our results. Specifically, the theoretical model of human capital transmission with nepotism where shocks are Cauchy is as follows. A potential scholar in generation t of family i is endowed with an unobserved human capital $h_{i,t}$. Human

Table H.7: Benchmark estimation under different set of moments.

Objective:		$V(p)$	$V_S(p)$
Panel A. Moments:			
Father-son correlations:			
Pearson, intensive margin	$\rho(y_t, y_{t+1})$	0.37	.
Spearman, intensive margin	$\rho_S(y_t, y_{t+1})$	.	0.38
Grandfather-grandson correlations:			
Pearson, intensive margin	$\rho(y_t, y_{t+2})$	0.22	.
Spearman, intensive margin	$\rho_S(y_t, y_{t+2})$	.	0.27
Distribution means:			
Father mean log-publications	$E(y_t)$	YES	.
Son mean log-publications	$E(y_{t+1})$	YES	.
Remaining distributional moments:		YES	YES
Panel B. Identified parameters:			
Intergen. elasticity of human capital	β	0.632	0.688
		(0.042)	(0.049)
Nepotism, %	γ	18.8	19.2
		(1.7)	(1.9)
Mean of human capital distribution	μ_h	1.715	1.292
		(0.432)	(0.605)
SD of human capital distribution	σ_h	3.880	4.003
		(0.182)	(0.255)
SD of shock to publications	σ_e	0.347	0.202
		(0.202)	(0.154)
Threshold of observable publications	κ	1.968	1.863
		(0.113)	(0.142)
degrees of overidentification		6	4

Notes: τ normalized to 0. S.E. between parentheses obtained by estimating parameters on 100 bootstrapped samples with replacement

capital follows a Cauchy distribution with location x_h and scale parameter s_h:

$$h_{i,t} \sim \text{Cauchy}(x_h, s_h)$$

The offspring of this generation, indexed $t+1$, inherit the unobserved human capital endowment under the first-order Markov process in Equation (2). The noise term $u_{i,t+1}$ is an i.i.d. ability shock affecting generation $t+1$, and has now a Cauchy distribution, Cauchy(x_u, s_u).

Human capital is stationary among potential scholars. That is, we assume that, conditional on the model's parameters being constant, the human capital of generations t and $t+1$ is drawn from the same distribution. Formally, $h_{i,t} \sim \text{Cauchy}(x_h, s_h)$ and $h_{i,t+1} = \beta h_{i,t} + u_{i,t+1}$ implies $h_{i,t+1} \sim \text{Cauchy}(\beta x_h + x_u, |\beta| s_h + s_u)$.[14] Stationarity leads to the following two restrictions:

$$\begin{aligned} x_u &= (1-\beta)x_h \\ s_u &= (1-|\beta|)s_h \,. \end{aligned}$$

Equations (4)-(5) give the publications for fathers, $y_{i,t}$ and sons, $y_{i,t+1}$ in the set of scholar families $\mathbb{P}$. The shocks affecting how human capital translates into publication now follow a fat-tailed distribution: $\epsilon_{i,t}$, $\epsilon_{i,t+1} \sim \text{Cauchy}(0, s_e)$.

Finally, the magnitude of nepotism, γ, is defined analogously to our baseline model. Formally,

$$\gamma = F_h^{cauchy}(\tau \mid h_{i,t+1} \geq \tau - \nu)\,,$$

where $F^{cauchy}(x;\ x_h, s_h)$ is the (stationary) Cauchy cumulative distribution of human capital with location x_h and scale parameter s_h, and $F^{cauchy}(x \mid h_{i,t+1} \geq \tau - \nu) = Prob\left(h_{i,t+1} \leq x \mid h_{i,t+1} \geq \tau - \nu\right)$ is the corresponding truncated cumulative distribution of sons' human capital in the set of observed scholar families $\mathbb{P}$.

There are three variants to the model of the main text (Model I). One with Cauchy distribution for all shocks (Model II), another with Cauchy distribution for shocks to human capital and Normal distribution for shocks to publications (Model III), and another with Normal distribution for shocks to human capital and Cauchy distribution for shocks to publications (Model IV). We evaluate Models II and III, as they lead to non-normal human capital distribution.

Table H.8 shows the results. The value of the objective to be minimized is higher when human capital shocks are modeled with a Cauchy. In other words, the data cannot be fitted well to a distribution with fat tails. For example, the gap between the 95th and 50th percentile for the son's log-publication distribution is 5 in the data (Table D.5), 5.8 in the simulation with the Normal distribution, and 6.7 (Model II) and 7.7 (Model III) in the simulations with the Cauchy. Finally, the nepotism estimates are robust to assuming Cauchy shocks, although the intergenerational elasticity β is not.

In sum, using fat tailed distributions for human capital shocks seems, *a priori*, an appealing alternative to the usual normality assumption. However, fat tailed distributions do not fit the data, which is very normally distributed after all.

[14] Because if $X \sim \text{Cauchy}(x_0, s_0)$ we have $kX + \ell \sim \text{Cauchy}(kx_0 + \ell, |k|s_0)$. And if $Y \sim \text{Cauchy}(x_1, s_1)$, $X + Y \sim \text{Cauchy}(x_0 + x_1, s_0 + s_1)$.

Table H.8: Identified parameters under different model assumptions.

Parameter		Model I	Model II	Model III
Intergen. human capital elasticity	β	0.688	0.268	0.438
Nepotism, %	γ	19.2	17.7	24.3
Std. dev. of shock to publications	σ_e	0.202	.	2.114
Scale of shock to publications	s_e	.	0.011	.
Threshold of observable publications	κ	1.863	0.844	0.062
Mean of human capital distrib.	μ_h	1.292	.	.
Location of human capital distrib.	x_h	.	0.880	0.102
Std. dev. of human capital distrib.	σ_h	4.003	.	.
Scale of human capital distrib.	s_h	.	1.054	0.889
Value of objective $V(p)$		476	4,355	3,351

Notes: τ normalized to 0; degrees of overidentification: 4

I Linearity of beta

So far, we assumed that parents with high and low human capital transmit their endowments at the same rate β. This linearity assumption would be violated, e.g., if successful fathers with many publications could spend less time with their children, reducing their human capital transmission systematically. Here we show empirically that, in our setting, the linearity assumption is satisfied.

To do so, we examine the parent-child elasticity of publications in the intensive margin. A large literature derives estimates of β directly from such parent-child elasticities. Here we compare elasticities obtained using OLS vs. estimated non-parametrically. The latter allow elasticities to differ in families with different levels of publications, and hence, with different human capital endowments.

Formally, our OLS elasticity estimates, b^{ols}, are:

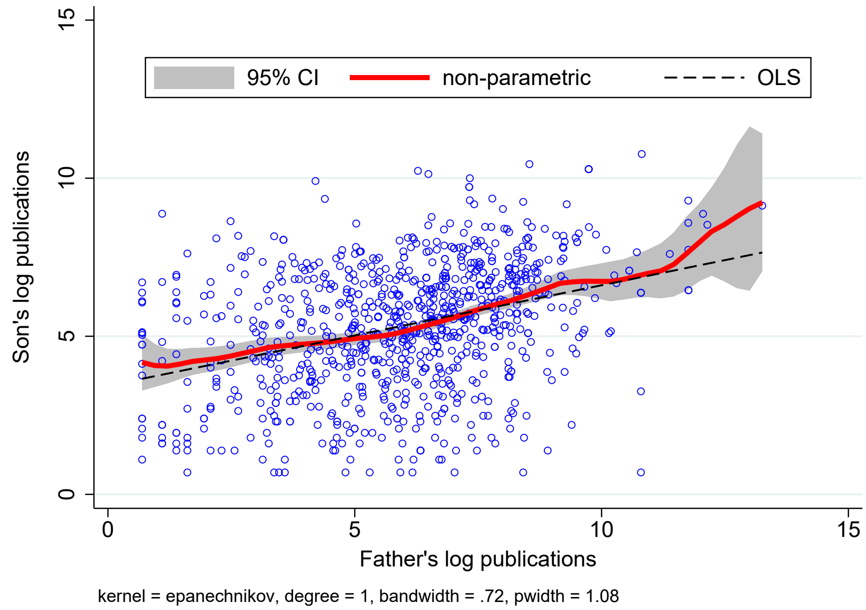
$$y_{i,t+1} = c + b^{ols}\, y_{i,t} + e_{i,t+1}\,, \tag{4}$$

where $y_{i,t+1}$ and $y_{i,t}$ are the log of 1 + publications for, respectively, sons and fathers with at least one publication in WorldCat. That is, b^{ols} is the publications' elasticity in the intensive margin. This specification assumes that b^{ols} is linear. Conversely, non-parametric estimates for the publication's elasticity, b^{np}, are:

$$y_{i,t+1} = g(y_{i,t}) + e_{i,t+1}\,, \tag{5}$$

where $g(.)$ does not follow any given parametric form but is derived from the data. Hence, this specification accounts for any polynomial form for $g(.)$, i.e., $g(y_{i,t}) = c + \sum_j b_j^{np}\, y_{i,t}^j$ for all $j \in \mathbb{Z}$. This allows elasticities to be different across families with different levels of publications. The non-parametric elasticity b^{np} corresponds to the marginal effect of $y_{i,t}$, obtained as averages of the derivatives.

Figure I.17: Parent-child publications' elasticity (intensive margin), robustness



Notes: The sample are fathers and sons with at least one recorded publication.

Figure I.17 compares OLS and non-parametric elasticity estimates. It shows a scattergram of fathers' (y-axis) and sons' (x-axis) publications, OLS fitted values from eq. (4) (dashed line), and non-parametric fitted values and 95% confidence intervals from eq. (5) (thick red line and grey area). Specifically, the latter plots the smoothed values of a kernel-weighted local polynomial regression of $y_{i,t+1}$ on $y_{i,t}$. To further capture non-linearities, we choose a polynomial of degree one for the smoothing. Finally, note that in this figure the OLS and non-parametric elasticities correspond to the slopes of the plotted lines.

Overall, the figure shows that there is no statistically significant difference between OLS and non-parametric estimates. This is true at all levels of father's publications. For fathers with fewer than 11 log-publications (more than $\leq 20,000$ in levels), the fitted OLS and non-parametric values are identical. In turn, the parent-child elasticity in publications (i.e., the slope of the lines) is tightly identified around 0.3 for both estimates. At the very top of the distribution, we also do not observe significant differences between OLS and non-parametric estimates, although the confidence intervals are wider due to fewer number of observations.

Table I.9 confirms this pattern for different historical periods. It shows the OLS (eq. 4) and non-parametric (eq. 5) elasticities for all families (row 1); for families before the Scientific Revolution (row 2); during the Scientific Revolution (rows 3 and 4); and during the Enlightenment (row 5). For all periods, the OLS and non-parametric estimates are almost identical.

Altogether, we find identical elasticities using OLS and non-parametric techniques. This suggests that the parent-child elasticity of publications is linear. In other words, it is identical for parents with

Table I.9: Parent-child publications' elasticity (intensive margin), robustness

	OLS [1]		non-parametric [2]		
All	0.32***	(0.03)	0.32***	(0.03)	N=887
Pre-Scientific Revolution (1088–1543)	0.01	(0.10)	0.02	(0.11)	N=73
Scientific Revolution (1543–1632)	0.24***	(0.07)	0.33***	(0.07)	N=177
Scientific Revolution (1632–1687)	0.33***	(0.05)	0.32***	(0.06)	N=244
Enlightenment (1688–1800)	0.39***	(0.04)	0.39***	(0.04)	N=393

Note: The sample are fathers and sons with at least one publication; SE in parenthesis; Non-parametric SE obtained with 1,000 bootstrapped replications; ***p<.01,** p<.05,* p<.1

high and low publications. This lends creedence to the assumption that human capital endowments are transmitted at the same rate β by parents with high and low human capital endowments.

J Heterogeneity in publication thresholds

The parameter κ is the minimum number of publications needed to observe a scholar's work in modern libraries. So far, we assumed that κ is the same for fathers and sons. An alternative is to assume that the threshold is lower for sons: the work of a famous scholar's son may capture the attention of publishers and librarians more easily—even if it is of lower quality. Here we examine the robustness of our results to this alternative assumption. We define the sons' threshold as κ_s, possibly lower than the father's threshold κ_f and estimate the corresponding model in Table J.10. We find that the constraint $\kappa_s \leq \kappa_f$ is saturated: our estimated κ_s and κ_f are almost identical. Hence, our estimation results are unchanged: we find similar intergenerational human capital transmission β (0.631 vs. 0.632) and percentage of nepotic sons γ (19.3 vs. 18.2%).

Table J.10: Results under alternative assumptions for κ.

Parameter		benchmark	different κ's
Intergenerational elasticity of human capital	β	0.632	0.631
Nepotism, %	γ	18.8	19.3
Mean of human capital distribution	μ_h	1.715	1.641
Std. deviation of human capital distribution	σ_h	3.880	3.927
Std. deviation of shock to publications	σ_e	0.347	0.188
Threshold of observable publications - all	κ	1.968	.
Threshold of observable publications - fathers	κ_f	.	1.943
Threshold of observable publications - sons	κ_s	.	1.938

Notes: τ normalized to 0; degrees of overidentification: 6

K Alternative measures of publications

So far, we defined publications as the number of library holdings by or about each scholar in modern libraries. We chose this measure because it captures two important characteristics of a scholar's work: its size and its relevance for today. Although we believe both characteristics to be important, it is interesting to examine the robustness of our results to measuring the size of a scholar's work without resort to its relevance for today. To do so, here we consider the number of *unique* works by or about each scholar instead of the total number of *library holdings*.

Table K.11 provides the empirical moments for our baseline measure (the log of 1 + library holdings) in column [1] and for the alternative measure (log of 1 + unique works) in column [2]. Panel A shows that the inter-generational correlations are very similar on the intensive margin, and are equal on the extensive margin by construction. That is, the high inter-generational elasticity (Fact 1) is visible both on the number of library holdings and on the number of unique works.

Table K.11: Targeted moments with alternative measures of publications

	Library holdings [1]	Unique works [2]
A. Intergenerational correlations		
Father-son, intensive margin	0.37	0.36
Father-son with zero pubs.	0.22	0.22
Grandfather-grandson, intensive margin	0.22	0.22
B. Father-son distributional differences		
Fathers with zero pubs.	0.29	0.29
Sons with zero pubs.	0.38	0.38
Fathers median	4.30	3.07
Sons median	2.83	1.84
Fathers Q75	6.68	5.09
Sons Q75	5.74	4.31
Fathers Q95	8.66	6.89
Sons Q95	7.85	6.08
Fathers mean	3.92	2.93
Sons mean	3.07	2.26

Panel B shows the moments characterizing father-son distributional differences. The levels are different by construction: the number of unique works is always equal or smaller that the total number of library holdings of these works. That said, the properties of the distribution and, especially, the father-son distributional differences (Fact 2) are robust. To see this, note that the father's median, mean, 75th and 95th quantile are higher than their sons' in both measures. To further show that the properties of the fathers' and sons' distribution are similar, Table K.12 shows quantile ratios. The median/Q75 ratio, the median/Q95 ratio, and the median/mean ratio are similar for fathers and sons independently of whether one uses library holdings or unique works as the measure of research output.

Table K.12: Comparison of distributions

		Library holdings (Baseline)	Number of unique works
Q50/Q75	Fathers	0.64	0.60
Q50/Q75	Sons	0.49	0.43
Q50/Q95	Fathers	0.50	0.45
Q50/Q95	Sons	0.36	0.30
Q50/mean	Fathers	1.10	1.05
Q50/mean	Sons	0.92	0.81

Table K.13 re-estimates our model targeting the moments defined with library holdings (baseline) and with unique works (alternative). Using the number of unique works, we find a β of 0.612, almost identical to our baseline estimate (0.632). Our main nepotism estimate, γ is also robust to the measure of publications. Specifically, our simulations based on the number of unique works suggest that, in 1088–1800, 18.5% of scholars' sons were nepotic. This estimate is also very similar to our baseline result of 18.8%.

Table K.13: Identified parameters with alternative measures of publications.

Parameter		Publication measure lib. holdings	nb. works
Intergenerational elasticity of human capital	β	0.632	0.612
Nepotism, %	γ	18.8	18.5
Std. deviation of shock to publications	σ_e	0.347	0.284
Threshold of observable publications - fathers	κ	1.968	1.529
Mean of human capital distribution	μ_h	1.715	1.412
Std. deviation of human capital distribution	σ_h	3.7880	2.907

Notes: τ normalized to 0; degrees of overidentification: 6

L Longevity

Longevity is an important factor for the number of publications of scholars. In our setting, scholars' fathers may have lived longer than scholars' sons. The reason is that, by construction, the former are recorded in our data conditional on living until they have a child, while the latter are recorded even if they die early after their nomination. In our sample of scholars with known birth and death year, the mean longevity is 67.7 (s.e 0.33) for fathers and 61.7 (s.e. 0.44) for sons. Here we show that this differential longevity does not affect our results.

To do so, we adjust the son's distributional moments accounting for the 6.0 year father-son gap in longevity. We do this in two steps. First, we calculate the marginal effect of living one additional year on the proportion of sons with zero publications and on the mean, median, 75th, and 95th percentile of the sons' log-publications. Second, we adjust the baseline distributional moments for sons by adding the marginal effects above times 6.0; the differential longevity between fathers and sons. That is, we calculate what the sons' distributional moments would look like if they had, on average, lived as long as fathers of scholars.

Formally, we first estimate the following equation by OLS:

$$y_{i,t+1} = \alpha + \delta(mean) \cdot L_{i,t+1} + e_{0\ i,t+1}\,, \tag{6}$$

where i indicates families of scholars and $t+1$ that the observation corresponds to a scholar's son; $y_{i,t+1}$, is the logarithm of 1 + the number of publications; and $L_{i,t+1}$ is the son's longevity, in years. Hence, $\delta(mean)$ captures the marginal effect of one additional year of life on the sons' log-publications. Estimating $\delta(mean)$ by OLS allows to understand this relationship for the *average* son.

We calculate analogously $\delta(zeros)$, the marginal effect on the proportion of sons with zero publications. That is, we estimate 6 by OLS where the dependent variable, $y_{i,t+1}$, is an indicator equal to one if a son had zero publications.

Next, we run a simultaneous-quantile regression to estimate the relation between longevity and publications at other distributional moments than the mean. Formally, we estimate:

$$Q_{y_{i,t+1}}(q|L_{i,t+1}) = \alpha_i + \delta(q) \cdot L_{i,t+1}\,, \tag{7}$$

where q is the quantile of interest; $\delta(Q50)$, $\delta(Q75)$, and $\delta(Q95)$ are the marginal effect of living one additional year on the median, 75th and 95th percentile of the sons' publication distribution; are all coefficients are estimated simultaneously

Table L.14 presents the corresponding estimates. Column [1] confirms that longevity is important for publications. One additional year of life is associated with an increase of 0.018 log-publications. Hence, if sons lived as long as fathers, their mean log-publications would increase by $5.8 \times 0.018 = 0.1$. Column [2] shows the corresponding marginal effect for the proportion of sons with zero publications. Note that this marginal effect is small and not statistically different from zero. This suggests that the high proportion of sons with zero publications is not a by-product of sons dying early after their nomination. This is important as our identification of nepotism partially hinges on father-son distributional differences at the bottom of the distribution. Finally, columns [3] to [5] show that one additional year of life is associated with an increase of 0.026 log-publications at the median and 75th percentile, and with an increase of 0.019 log-publications at the 95th percentile of the sons' log-publications distribution. Hence, if sons lived as long as fathers on average, their log-publications would increase by $5.8 \times 0.026 = 0.15$ at the median and 75th percentile; and by $5.8 \times 0.019 = 0.1$ at the 95th percentile.

Table L.14: The effect of Longevity on son's distributional moments

	[1]	[2]	[3]	[4]	[5]
	OLS	OLS	simultaneous-quantile regression		
	$\partial(mean)$	$\partial(zeros)$	$\partial(Q50)$	$\partial(Q75)$	$\partial(Q95)$
Longevity (years)	0.021***	-0.0009	0.028***	0.029***	0.016**
	(0.005)	(0.0007)	(0.006)	(0.004)	(0.007)
Observations	1,267	1,267	1,267	1,267	1,267

Note: The sample is scholars' sons with known birth and death year; $^{***}p<.01$, $^{**}p<.05$, $^{*}p<.1$

Finally, Table L.15 shows the adjusted sons' distributional moments. Column [1] shows the baseline moments and column [2] the adjusted moments if scholars' sons had lived as long as scholars' fathers. The adjusted moments are $m + \partial(m) \times 6$; where m is the baseline value, $\partial(m)$ the marginal effect of longevity at moment m, and 6 the father-son differential longevity. The baseline and adjusted moments are very similar. The proportion of sons with zero publications (0.38) is not altered by adjusting for the fathers-sons longevity differential. The mean, median, 75th and 95th percentile of the sons' log-publications are larger when we impute the same longevity to sons and fathers. For example, if sons lived as long as fathers on average, their mean log-publications would have been 3.19 instead of 3.07—which corresponds to an increase of 0.12 log-publications. That said, the adjusted distributional moments are consistent with *Fact 2*. After accounting for longevity differentials, the publication's distribution of fathers first order stochastically dominates that of sons. On the bottom of the distribution, 30% of fathers and 38% of sons had zero publications, even after accounting for longevity differentials. These distributional differences are also visible at the mean, median, 75th and 95th percentile. For example, the average father had 4 log-publications (54 in levels), more than twice as much as the average son (21 in levels) even after accounting for longevity differentials. The father-son differences at the median are reduced by 0.17 log-publications after adjusting for longevity, but the median father still published ca. 3.5 times more than the median son. Importantly, this implies that, after adjusting for longevity differentials, the father-son distributional differences are relatively larger at the bottom of the distribution than at the mean or at the median.

Altogether, the evidence suggests that longevity affects publications, but that father-son longevity dif-

Table L.15: Distributional moments adjusted for longevity differentials

		Baseline [1]	Adjusted [2]	Difference [2]-[1]	N
Fathers with zero pubs.	$\Pr(y_t=0)$	0.30	.	.	1,538
Sons with zero pubs.	$\Pr(y_{t+1}=0)$	0.38	0.38	0.01	1,748
Fathers median	$Q50(y_t)$	4.32	.	.	1,538
Sons median	$Q50(y_{t+1})$	2.83	3.00	0.17	1,748
Fathers 75th percentile	$Q75(y_t)$	6.75	.	.	1,538
Sons 75th percentile	$Q75(y_{t+1})$	5.74	5.92	0.17	1,748
Fathers 95th percentile	$Q95(y_t)$	8.65	.	.	1,538
Sons 95th percentile	$Q95(y_{t+1})$	7.85	7.94	0.10	1,748
Fathers mean	$E(y_t)$	4.00	.	.	1,538
Sons mean	$E(y_{t+1})$	3.07	3.19	0.12	1,748

ferences do not explain away father-son distributional differences (*Fact 2*). This shows that our estimates for nepotism and the intergenerational human capital elasticity are not driven by differences in longevity.

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